

PHILOSOPHICAL
OBSERVATIONS
ON THE
S E N S E S
O F
VISION AND HEARING;
TO WHICH ARE ADDED, A TREATISE
ON HARMONIC SOUNDS,
AND AN ESSAY
ON COMBUSTION
AND
ANIMAL HEAT.

John

BY J. ELLIOTT, APOTHECARY.

L O N D O N :

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T O
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ROYAL COLLEGE OF PHYSICIANS,
LONDON,
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OF THE ROYAL SOCIETY OF MEDICINE AT
PARIS;
A GENTLEMAN
NOT LESS DISTINGUISHED BY
HIS HUMANITY
AND SUPERIOR SKILL IN HIS PROFESSION,
T H A N
BY HIS EXTENSIVE ACQUAINTANCE WITH, AND ZEAL
TO PROMOTE EVERY BRANCH
OF USEFUL KNOWLEDGE.
THE FOLLOWING SHEETS
(AS A SMALL, BUT SINCERE TRIBUTE TO HIS MERIT)
ARE RESPECTFULLY INSCRIBED
BY HIS OBLIGED,
AND DEVOTED HUMBLE SERVANT,

*Garnaby Market,
Nov. 4, 1779.*

J. ELLIOTT.

E R R A T A.

Page iv. line 13, for *chin* read *upper lip within the face*.
 Page xii. line 4, for *is* read *be*. Page xx. line 7, for *excite*
 read *excited*. Page xxiii. line 19, for *at* read *of*. Page lxi.
 line 14, for *mean* read *result*. Page lxxviii. line 1, for *of*
 read *for*. Page lxxxviii. line 10, for *inflammable* read *unin-*
flammable. Page cxli. line 18, for *with the bodies* read *with*
bodies. Page cxix. line 10, after *air*, read *more than by one*
another. Page clxxxv. line 14, omit *more*. Page clxxxvii.
 line vi, for *philologists* read *physiologists*. Page cxc. line 12,
 for *walking* read *waking*. Page ccxi. line 2, after *that* read
either atmospherical or dephlogistified. See also page cxi, &c.

Note. To what is said in the Appendix relative to the four
 principles, may be added, that particles of fire seem to attract
 æther more than they do one another, and more than they do
 particles of earth. And that particles of phlogiston attract
 æther more than they do one another, but less than they do
 particles of earth. The reasons for this opinion will easily be
 perceived by those who read the last Essay.

P R E F A C E.

THOSE who have any acquaintance with the Press know that the first sheet of a book, containing the title page, &c. is generally printed last. This remark may account for some passages in the sequel.

THE first of the following Essays I have had by me many years, and have my reasons for mentioning that it is taken from a folio manuscript containing many other inquiries, which was in the hands of persons, not my friends, so long ago as the year 1772; the greatest part of that manuscript was written long before: and the substance of the Essay alluded to, has been in the possession of a respectable philosophical character near three years, though it has not till now been convenient for me to make it public.

IN the second Essay, I forgot to mention that the note 1, in the scale, page 79, for the violin,
may

may be called C ; and so in proportion for the larger instruments ; or else the name may be varied by tuning higher or lower, as mentioned in the Essay. The strings fitted to the violin may be very small fourths, or good thirds of the lesser size. I once fitted a guittar with six, and afterwards with eight strings, regularly tuned on the idea of that scale, and convinced myself that the hint might be prosecuted to advantage on viols. By tuning eight strings according to the musical intervals of an octave, the compass of a fifteenth may be obtained, without having recourse to any of the difficult frets, or less melodious notes. And the same compass (which is sufficient for most performances) may be commanded with only six strings, by taking in notes above the one fourth fret, which notes will still be good, and not difficult to hit. Six strings can easily be managed with the bow : for greater nicety in the execution, the frets may be marked ; and perhaps it will be found more convenient to use the notes on the two-fifth division, instead of those on the one-fifth, as there will then be no occasion to shift the hand. An instrument of this kind might either be played by itself, or used to strike in
the

the harmonic notes occasionally in a concert, &c. where their effect would be much finer, than when only drawn from a common instrument tuned fifths, as would be found on trial. I did not think to insert a scale for the viol di gamba; but it may easily be constructed by those who understand the theory. It may be remarked, that octave notes *on the same string*, are rather *bearing*; and the like may be observed of the other intervals: the reason will be obvious to those who read that Essay.

IN regard to the last Essay, I have been a little unfortunate, as a most excellent treatise has been published on the subject by Mr. Crawford since mine was in the press*: those who have read that truly admirable work, will therefore find some errors, which otherwise perhaps they would not have known to have been such, at least for the present. My performance may be compared to the first dawn of twilight; Mr. Crawford's to meridian sunshine: and it is rather an unlucky circumstance that the *latter*

* It was no fault of the Author (as could be made appear were it necessary) that this work was not published in June last.

should have appeared before the *former*. Yet it ought to be remembered how highly Des Cartes' philosophy was admired, even by the most learned, before Newton's Principia appeared. It may be added, that Mr. Crawford had the advantage of being able to prosecute the inquiry by experiments; whereas I, having been less eligibly situated in life, could only proceed by mere speculation, or guess-work.

For the same reason, I have not been able to pay that attention to the style which is necessary to a work intended for the Public. The ill-natured critic will ask, why then did I publish at all? I answer, that I should not have troubled the world with these Essays, if better judges than me had not thought that they contained hints which those who have leisure and proper conveniencies might improve into real discoveries: a section * in Dr. Priestley's celebrated History of Electricity will sufficiently justify my conduct in this respect.

* Part IV. section I.

OBSERVATIONS

ON THE

S E N S E S.

SECTION I.

O F V I S I O N.

THE uses of the several humours and coats of the eye, and the manner in which light is refracted by them for forming the images of objects on the retina, have already been explained by philosophers. My design is, to endeavour to show in what manner light acts after it arrives at the retina, for causing the appearance of lumination or colour. This branch of optics has also been cultivated; but as a consideration of what happens in this sense, will serve to illustrate what I have to advance concerning the ear, I shall enlarge on it, regardless of what may already have been written on the subject, especially as my observations are of a new kind; or at least have not hitherto been so fully attended to.

IT has been frequently observed, that if the corner of the eye be pressed with the finger, a luminous spot or ring will appear, somewhat differently coloured; and that from a stroke on the eye, an appearance like a flash of fire has sometimes been observed.

AFTER repeated trials, and putting myself to some pain, I learnt that by pressing the balls of my eyes with my hands, in the direction of their axes, with as much force as I could bear, keeping them steady, and assisting the pressure with the strong compression of the lids, and contraction of the neighbouring muscles, there would, after some time, appear a large luminous sensation like a concave hemisphere of light, but not very lucid, and chequered (often in a very regular manner) with dark and less lucid intervals. If the pressure be continued, and the eyes winked very strongly, the appearance will be much brighter, and will seem to tremble. There will sometimes also appear large crooked streaks of light, much brighter than the other parts, and with certain vermicular, or eel-like motions. By increasing the pressure till the eyes become quite hot and red, the light will
be

be at the brightest, and almost as lucid as at noon day : till this time the appearance is generally of a whitish colour, tinged with yellow, or orange, like the sun ; or rather like the light of the moon, or a candle. By continuing the extreme pressure, the brightness of the appearance begins to decay, and the colour gradually changes from a reddish and yellowish white, to a bluish one ; and sometimes several kinds of coloured spots will appear, as red, green, blue, and a fine violet which generally disappears the last, and those more verging to red soonest ; for now the light totally vanishes, nor can it be recalled by a continuation or increase of the pressure. If now the hands be removed, and the eyes opened, they will be quite blind even to the direct light of the sun ; and it will be some time before they recover their sight, and then but by degrees.

THIS experiment is very painful, and it is not every one that would choose to repeat it after me with the requisite care. Before I proceed farther, it may be necessary to clear up some particulars which are to be met with in the above account.

THE luminous appearance seems like a concave hemisphere, or parabolic conoid ; it seems to surround the face only, and not the whole body. For example, if I press the corner of either eye, a ring of light will appear on the side of the face opposite thereto ; and by removing the finger at intervals all round the margin of the eye, the ring will appear to move all round the face : that is, when the finger is on the right side of the eye, the spot or ring will appear on the left side of the face ; and contrariwise. Also, when the finger is above the eye, the spot appears as if it was about the chin ; and when the finger is under the eye, the spot appears as if upon the forehead ; but in all these cases, provided the edge or margin of the retina be pressed, it seems as if it was close to the face. So when the lumination is excited in the manner above described, the margin of it seems to touch the face, but the centre of it appears as at some distance from the face, and the distance is less as you approach towards the margin ; which gives the whole the resemblance of a concave segment of a sphere, or parabolic conoid, as mentioned above. Also the two eyes form but one hemisphere, as I know by causing
this

this lumination by pressing one eye only. Now as these are affairs (I think) merely mechanical, or resulting from organization, and in which I could not have been misled by custom or habit, as is the case in many instances of common vision, these appearances having occurred at the very first time of exciting the above lumination, may they not be made use of to settle the famous disputes concerning the inversion of images in the eye, and seeing singly with both eyes? Has not every part of the retina of one eye an answerable part in the other? Do not the corresponding fibres of the right sides of both eyes meet in the brain, and terminate in the left side of the sensory? Those in the left sides of both eyes, in the right side of the sensory? and those in the upper and lower parts of the retinae of both eyes in the contrary parts of the sensory, so as to be in an inverted situation in the latter, to what they are in the former *?

BUT

* The most decisive experiment that I have met with against the junction of the respective fibres of both eyes is that blue and yellow thrown separately on the answerable parts of both

BUT it is most worthy of remark, that by pressing the margin of either eye, the ring appears not as on the opposite side of that *eye*, but of the *face*. Also the concavity of the luminous appearance which ariseth on pressing the centre of the eye, and its surrounding not the *whole body*, nor merely the *eye*, but the *face*, shew that the retina in the brain encompasses the whole of that portion of the sensory which answers to the *face*.

THE luminous appearance which ariseth on pressing the centres of the eyes, as described

retinae do not cause a green. The corresponding fibres are not perhaps, united, because then if one nerve was disordered or destroyed, the other would also. They may only run by the sides of each other, or even in contrary directions, and terminate in the brain so as to form two different surfaces, concentric, and at a small distance from each. And this seems to appear by certain circumstances of the chequers in pressing both eyes; in other respects the opinion seems to be well founded. Thus any two answerable parts of the eyes, whether excited by the rays of light, or by pressing the corners or centres, yield but one sensation, as if but one eye had been affected; excepting that the sensation is stronger. In squinting eyes the ball is perhaps distorted with respect to the retina, which may account for any seeming deviation, in these instances, from the above rule,

above,

above, is chequered ; that is, some parts of it are darker than others, and sometimes there appear spots, and streaks which are much brighter than the other parts of the lumination. The causes of which I take to be that the surface of the retina is not even or smooth, but has prominencies or ridges answerable to the regular form of the chequers, and which may result from its structure * ; so that the vitreous humour must press on it unequally, and by that means cause some parts of the appearance to look brighter, and others darker : for the cause of the lumination is in the retina, as will presently be shewn. The apparent trembling I take to proceed from the trembling of the eye and retina, on account of the violent pressure ; the moving vermicular streaks may arise from the convulsion of the membranes, or coats of the eye impressed on the retina, and perhaps also from the convulsions and tremblings of these parts themselves ; the blindness ariseth, I suppose, from the universal oppression of the fibres of the optic nerve expanded in the retina, and this paralysis of them I take to be the reason

* From its net-like structure it derives its name (*Retina*).

why the eyes do not recover their sight immediately, but by degrees; because it must be some time before the nerves can be relieved from their oppression, so as again to communicate the action of the rays of light on them freely to the sensory.

WHEN the corner of the eye is pressed with the finger in the usual way, there does not appear an universal lumination, as on pressing in the direction of the optic axis, but only a small ring or spot of light about half an inch in diameter. There is also this difference between them, that the spot caused by the lateral pressure presently disappears, unless the eye be struck repeatedly, or with a quavering motion; whereas the lumination excited by pressing the centres of the eyes, continues without requiring such quavering motion. The reason of this difference I take to be, that in the first case only a small part of the globe of the eye is protruded on the retina by the oblique pressure, and of course only a small spot, proportional thereto, is excited in the sensory. The reason why it so soon disappears is, I take it, because the globe being soft, and pressed but obliquely,

liquely, gives way, and changeth its figure so as to make the pressure on the retina equal ; whence the partial pressure made by the protrusion on the retina there being removed, the luminous spot disappears ; but if the finger be lifted up, or the pressure lessened, the globe presently recovers its figure ; and if struck by the finger again, the same partial protrusion, and luminous spot arising therefrom will be caused, but which will again presently disappear, by reason of the globe changing its figure as above ; and hence the necessity of the quavering motion of the finger to preserve the ring *. But these things do not happen on pressing the centre of the eye, because there is then no room for the globe to elude the pressure, which in this case is therefore general ; so that the lumination once excited, continues as long as the pressure re-

* By pressing the corner of the eye harder than usual, I have sometimes excited two rings, one on each side of the face ; that at the side opposite to the corner pressed arose in the usual way ; the other, I imagine, from the effect of the pressure passing diametrically through the globe, and causing a protrusion on the opposite part of the retina.

mains,

mains, or till the nerves become paralytic. And even if the lateral pressure be continued and increased for some time after the luminous ring disappears, there will arise an universal lumination, as on pressing the centre of the eye with the ball of the hand, and for the same reason. But the experiment is so painful that it cannot be made to advantage.

THESE experiments are best made in the dark and in bed. Before and after sleep, or fainting, the lumination described above appears in some measure even without pressing the eye. Luminous sparkles are also perceived by a person in a dark room, caused merely, I suppose, by the action of the circulating fluids, &c. on the retina.

Now the general conclusion which I would draw from what has been said is, "That colours are liable to be excited in the eye which do not at all depend on the rays of light." A conclusion which I imagine no person, who properly considers the experiments, will refuse me.

FROM

FROM the analogy discovered by Sir Isaac Newton between the vibrations of the rays of light, and those of musical strings, or of the pulses of air for causing musical sounds, it is concluded that these rays cause vision by means of their vibrations; and that the different colours, like notes of music, depend on the different times of the vibration: may we not therefore infer, “that since colours may be excited in the eye, independent of the pulses of the rays of light, they are caused by vibrations liable to be excited in the eye, of the same times as those of the rays of light? And that as there are different kinds or tones of colour, there are also as many different times of vibration for causing them?” For the luminous appearance which ariseth on pressing the centre of the eye, is in general white like the sun’s light, or rather like that of the moon, or a candle. But such a colour as this is found by refracting light to be, not an original or simple colour, but compounded of others; as red, yellow, green, blue, and violet, with all their intermediate shades answerable to the degrees of refrangibility. And by parity of reason, since the
like

like white colour ariseth on pressing the eye, this also cannot be a simple colour, but one composed of the same ingredients; that it is so appears by the experiment above described; for there some of the ingredients appeared either separate, or much less compounded. And even the white varies its colour, being sometimes a reddish or yellowish white, and at other times verging to a blue. The ring or spot which appears on pressing the corner of the eye, is likewise sometimes variously coloured, as others have observed. Also if this ring be made very luminous, it is tinged with yellow or red; but if it is faintly excited, it is rather inclined to a greenish blue, as is the case in the central pressure. Now, by applying the old maxim of philosophers, that "Nature does nothing in vain," may we not be allowed to draw the following conclusions? viz. "That the rays of light could not conveniently be made to communicate their vibrations immediately to the nerves, but that the interposition of those shewn to exist in the retina was necessary to that end. That therefore there are in the retina different times of vibration liable to be
excited,

excited, answerable to the times of vibration of the several sorts of rays. That any one sort of rays, falling on the eye, excite those vibrations, and those only which are in unison with them, not at all affecting the others, and therefore cause only their proper colour. And that in a mixture of several sorts of rays, falling on the eye, each sort excites only its unison vibrations, whence the proper compound colour results from a mixture of the whole.

N. B. As the following remark relates to vision, it may be here subjoined.

MARRIOTTE has published a curious experiment which shews that there is an insensible spot in the retina, at the entrance of the optic nerve. If an object be looked at whose image occupies the whole surface of the retina, one would imagine from thence that an hole or dark spot should be perceived in the part of the object answerable to the insensible spot in the retina; but no such spot or hole is seen. I have observed that images which fall near
that

that spot are not perceived as properly defined. In the concave lumination excited by pressing the centre of the eye also, no such spot is discernible. There is no such spot or vacuity therefore in the retina of the sensory (if I may be allowed the expression) ; it seems to be filled up by the fibres of the optic nerve dispersed around the spot in the eye : hence the ill defined images there.

S E C T.

SECTION II.

OF TASTE, SMELL, AND FEELING.

SINCE the discovery of the analogy between colours and sounds, the various kinds of tastes and smells have been considered as so many different tones or notes of these sensations. In what manner liquids and odours act on the organs for the purpose of causing the respective sensations, whether by vibration or otherwise I pretend not to determine. All that I have to observe on the subject is, that with regard to taste and smell, the case is not the same as hath been shewn with respect to the eye: that is, "There are no innate tastes or smells liable to be excited by pressing or irritating these organs, as is the case with the colours in the eye;" at least I have not been able to discover any such by the experiments that I have made: and therefore, if odours and liquids cause tastes and smells by means of vibrations, they must act immediately on the nerves.

WHETHER

W H E T H E R feeling, or pain, be caused by vibration, or in what other manner, I also cannot determine, though I suspect the former. I have only to observe, that the manner in which this sensation is ordinarily excited is different from what happens in the eye. Vision is usually excited by unison vibrations, though it may be caused by pressure or irritation, as hath been shewn: “but feeling or pain is ordinarily excited by mere irritation or pressure.” In which respect it also differs from tasting and smelling.

N. B. T H E senses are usually reckoned but five; I would add thirst, hunger, heat, cold, titillation, &c. But of these I have nothing to say but what may be collected from the observations on tastes, smells, and pain.

SECTION III.

OF HEARING.

I HAVE purposely deferred the consideration of this sense till last, because there are some particulars of the others which will be proper for illustrating what I have to advance concerning this. It appears that, in the different senses, different modes of operation of the respective agents take place. In the organs of taste and smell, liquids and odours communicate their action immediately to the nerves. In feeling, or pain, nothing but irritation is employed. But vision, though it may be excited by mere irritation, as hath been shewn, and as is the case with the sense last mentioned; yet for obtaining the shapes of objects, and for other purposes of seeing, it was necessary to employ the rays of light; which yet do not act, like liquids and odours, immediately on the nerves, but by the mediation of unison vibrations. It has, I think, been generally supposed, that the case with the sense of

hearing is similar to what has been said concerning taste and smell, viz. that the vibrations of the air are communicated immediately to the auditory nerve. But that this is not true, may perhaps be concluded from the following observations.

It has been remarked by others, that when a person is sleepy, or tired, when the ears are swelled by means of a cold, &c. before and after sleeping, or fainting, in gaping wide, and on other occasions; but particularly when the ears are violently struck, a noise is sometimes heard, called vulgarly “a ringing or ringing in the ears;” having observed it in myself, and particularly twice, when I heard several distinct musical sounds, I conceived the phenomenon to be worthy of notice, and proceeded to examine it. It would be tedious to recount the methods which I practised, and the pain I sometimes put myself to in order to find out a method of exciting these sounds. Let it suffice, that by strongly contracting the muscles on the sides of my head, and thrusting my fingers into my ears, pressing different parts of them, more easy to be learnt by experiment than description, I could
at

at any time excite them, in a confused medley, to a great number. And if I made the experiment when warm in bed, and inclined to sleep, I could at length, merely by pressing my finger on different parts of my ear, excite some of them in a manner sufficiently distinct to be considered as if separate or alone, and by that means make out a kind of plain tune. And even when they are excited in the most confused manner, some may be attended to and considered independent of, or distinct from, the rest; as is the case when many sounds are heard together in a concert. By pursuing my inquiry, I found that they had the following properties.

I. THAT these sounds are not audible in the natural state of the ear, but require distension or pressure, or, more properly speaking, *irritation* to excite them.

II. THAT the loudness or strength of the sound might be increased by increasing that irritation.

III. THAT the same sound never varies in tone or note;—as I knew by comparing several

ral of them which I could excite with certainty at pleasure, with the notes of a fixed musical instrument, with which I at first found them in unison; for I could never afterwards find any sensible difference between them.

IV. THAT they do not at all depend on the pulses of the air;—for if I excite them ever so strongly they could not be heard by another person; which, from their loudness, must have happened had they been the effects of aërial pulses.

V. THAT there are a great number of them.—The exact number, or latitude of their scale, I have not yet been able to determine, because of the difficulty attending the subject. It requires practice and patience to excite them at all to advantage: the ears must be compressed by the muscles around; the mouth occasionally opened or shut; a gaping raised; the fingers pressed not only at different parts of the hole, but also around the external ear *; the

* By thrusting the finger into the ear, and then withdrawing it, so as to cause a vacuum in the manner of a syphon, the sounds are excited to great advantage, and in this way it was that I discovered the low ones,

meatus

meatus auditorius may be filled with water: all these circumstances should be occasionally varied; but above all, the times most favourable for exciting them, and a place free from noise should be chosen. In short, it will be much better learnt by practice than description; for I imagine that different means are required in different people, because I find a very great dissimilarity in this respect between my right and left ear: in the latter I can much more easily excite them than in the former, neither can I always excite unison sounds in both ears by the same means. And here it may be proper to remark, that persons of a light turn of mind, and but superficially acquainted with philosophical matters, may ridicule these kinds of experiments, and laugh at an attempt to deduce a theory of hearing from "a ringing in the ears." But the case, I presume, will be different with those of another cast, who examine the matter with due attention, and reflect, that the most important discoveries in philosophy have been suggested by the most trifling and even childish phenomena. For a long time I could excite no sound in my left ear deeper than what answered to the middle D of a German flute. I

have since gone as low as B; but in my right ear I can now go full two notes lower, viz. to G. As it is with the utmost difficulty that these low notes can be raised, it should seem that there are others still deeper, but which are not excitable by the means above described. Also in my left ear, I can raise notes from B to about an octave above, in all the intermediate gradations, or sensible differences; but from thence, to a great part of another octave, I cannot yet excite them, though still higher they may be raised in great plenty, but in a more confused manner; and they seem also as if they were in a different part of the ear, or more inwardly than the lower ones. From these considerations, it seems to appear that there is a regular gradation of them from the lowest to the highest; though, on account (I suppose) of the particular structure of the ear, and my being able to press it only externally, I cannot yet excite them. In the right ear, several sounds, intermediate to those just mentioned, are distinguishable; and in both ears I can excite many sounds which are evidently unisons to each other. A rumbling noise generally heard in making these experiments is not to be

be confounded with the sounds above described, but to be considered as arising from a vibration of the internal air, communicated to it by the motion of the tympanum, as will hereafter be described.

Now, from the above experiments does it not appear (analogous to what has been shewn with regard to the eye) that “there are sounds liable to be excited in the ear, which do not at all depend on the pulses of the air?”

IF this be granted, may we not extend the analogy farther, and reason in the manner following?

IT is demonstrated by philosophers, that sounds are caused by tremors or vibrations in the air. And therefore, “since sounds may be excited in the ear, which do not at all depend on the pulses of air, they are caused by vibrations liable to be excited in the ear, at the same times as those aërial vibrations which cause the same sounds. Also, as there are many different notes or gradations of these internal sounds, there are as many different vibrations liable to be

excited in the ear for causing them." The uses of these sounds may likewise be presumed to be analogous to what was shewn of the innate colours; viz. "That the air, external or internal, could not conveniently be made to communicate its vibrations immediately to the auditory nerve, but that the interposition of those shewn to exist in the ear, was necessary to that end. That, therefore, there are in the ear different times of vibration liable to be excited, answerable to those of the air, for causing the several gradations of sound *. That any one time of ærial vibration, acting on the ear, excites only that internal one with which it is in unison, not at all affecting the others, and therefore causes only the answerable sound. And that in a mixture of several sorts or times of ærial vibration beating on the ear, each sort excites only its unison vibration, as was shewn to be the case with regard to colours."

* The scale of audible sounds is said to be about eight octaves; and the lowest sound is caused by a vibration at the rate of thirty in a second,

SECTION IV.

*Concerning the Manner in which we obtain an
Idea of the situation of sounds, and other
Phenomena of*

H E A R I N G.

MY design in this Essay was to establish the doctrine of Internal Sounds, as delivered in the last section. With regard to the subject of this, I confess I have nothing to offer which I can satisfy myself of the truth of by experiment, and would only wish to excite the attention of philosophers to so curious an inquiry.

OBSERVATION I. The sounds which are excited by pressing the ear, are weak in comparison to the loudness with which they may be excited by the vibrations of the air; and yet it is well known, that the sound of a musical string is much more easily excited by striking it, than by an unison vibration. Hence we find,

find, that, in the ear, proper methods are employed for increasing the force of the pulses of these vibrations. I pass over what share the tympanum, the bones, &c. may have in this intention, and shall only observe, as others have before me, that the labyrinth and cochlea are contrivances similar in principle to the very means which we usually employ for the purpose of increasing the loudness of sounds. One design of the structure which we observe in the ear, therefore, seems to be to increase the force of the pulses of ærial vibrations, the better to enable them to excite the internal sounds.

OBS. II. If the centres of both eyes are pressed, only one coneave appearance is formed; which ariseth from hence, that every part of the retina of one eye has a corresponding part in the other, as was shewn in the first section. but if the ears be pressed, the sounds do not appear to be thus united, but those of each ear are heard as on the respective side of the head. This difference is the more remarkable, because experience shews that we hear singly with both ears, even as we see singly with both eyes.

Obs. III. In exciting the innate colours, a wide circular scene appears, in which, as in the retina, objects may be placed in different situations with respect to each other: but nothing answerable to this can be observed on exciting the innate sounds; neither does the cochlea or labyrinth appear to be at all adapted to such a purpose.

Obs. IV. Sounds may be excited in the ear by the vibrations of the air though the tympanum and little bones be destroyed, as hath been observed by others. The cochlea and labyrinth, therefore, are properly the ear, as the retina is properly the eye; and the tympanum, bones, &c. are only appendages subservient to certain conditions of hearing, as the humours of the eye are for seeing.

Obs. V. It has been thought by some, that ærial sounds, from whatever quarter they come, affect the same parts of the ear, because vibrations in the air are supposed to be propagated alike on all sides. We readily judge, however, at first hearing, from what quarter without us a sound comes; and this is so true, that

(as

(as in optics) a sound is heard as in that place or from that quarter from whence it was last reflected: and when walls, or hills, are duly posited, the original and the reflected sounds are both heard as in their respective places. So in a concert, we hear many sounds distinctly from each; and not that only, but we judge immediately from what quarters around us the different sounds heard at the same time come. Yet, whoever considers the structure of the cochlea and labyrinth, and the general, confused, and similar manner in which sounds must enter them, from whatever part they arrive, must own, that they do not seem at all calculated to answer to these phenomena. After the most attentive consideration, it seems to me that the tone or note, and the strength or loudness, are the only parts of hearing with which sound is concerned.

Obs. VI. I had often wondered why the malleus should be fixed to the tympanum, and that a cord, or nerve should run across that membrane behind, because they seemed to me to hinder its vibration. Nature, however, has a reason for every thing she does, and therefore

fore some purpose is undoubtedly answered by them. It has since occurred to me, that if the tympanum had been free, like the head of a drum, it would, like that, have been capable of yielding only one sound or note at a time *; and therefore for every different note it must have been proportionally tightened or relaxed. But on the contrary, we can hear several, or many different sounds at the same time, and even judge of their situations, as observed above; which perhaps could also not have obtained if the tympanum had been free.—Are not ærial pulses, according as they come from a different quarter, made, by means of the external ear and meatus auditorius, to beat upon a different part or side of the tympanum?—Do not the contrivances abovementioned prevent the vibration being uniformly communicated to the whole membrane, and confine it to the part immediately affected?—Any part being passive to, or liable to receive, any time of vibration that the air may impress on it, and dif-

* Whether this holds with regard to receiving and transmitting of sounds from the air, as well as by beating it with a stick may be doubted. It would only perhaps be done less clearly or distinctly when free, than otherwise.

ferent vibrations, in different parts, at the same time ?

Obs. VII. By repeated experiments, I find that the internal sounds are not in the tympanum. When I touch that membrane with a probe, I find indeed that it has a motion, and that motion occasions the rumbling noise which ariseth on exciting the internal sounds, as before observed. But I take it that it occasions that noise only by acting on the internal air, and thereby exciting the answerable innate sound. This motion I imagine is caused by an alternate action of the muscles of the malleus, which pull it to and fro, for it is a motion of the whole membrane: it seems to be performed but very few times in the course of a second, and is always, as far as I can find, about the same swiftness, though the strength or latitude of the motion is very variable. I suspect that this motion of it is continual in some degree; for if a wilk-shell, or other hollow body be applied to the ear, a noise is heard like the waves of the sea; which is a very common experiment, and seems to proceed from the motion of the air (in consequence of such action
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of the tympanum) reflected and made sensible by the hollow body so applied. I repeat, however, that the internal sounds are not in the tympanum: neither does it appear to me that the whole tympanum, or the muscles of the malleus, are by any means capable of alternate action so swift as the vibration of aërial sounds, some of which are at the rate of several thousands in a second. Considerations like these have led me to suspect that the motion of the whole tympanum, above described, and of the little bones, have less share in the business of sound or actual hearing than has been commonly imagined *. Are they not either wholly or chiefly subservient to the purpose of exciting the passions, and other affections of the mind and body, which we find, by experience, to be the consequences of certain conditions of aërial sounds?

OB S. VIII. The tympanum is so very sensible that we cannot well make experiments on

* It has been thought that the whole tympanum is put into vibration by every different sound; but this will appear absurd, when two or more sounds are acting on it at the same time.

it. Yet if any one chooses to try, he will find, by touching it with a probe, that the sensation does not seem to be confined to that point, but to affect the system in a more general manner. If the motion of the whole tympani of both ears, described above, be sensibly excited, especially if the ears be closed, the sensation seems to fill or surround the whole head. I had a patient (to whom I can refer the doubtful) who appeared, by the symptoms, to have had a suppuration in the barrel of the right ear; for putrid matter was afterwards discharged from thence into the mouth. If this patient leaned her head forward, she felt, as she expressed herself, *her brains fall forward*, and if afterwards she held it backwards, she thought she felt *her brains fall backwards again*; which made her fancy that her brain was loose in her skull; but if she lifted her head up, by placing her fingers about that ear in a particular manner, those things did not happen; and thence she thought her brain, during that time, was restored to its right place. From the last-mentioned circumstance, the cause of this seeming affection of the brain was undoubtedly in the ear; perhaps one of the little bones had been loosened

loosened by the suppuration, or matter may have floated in the cavity: hence, according as her head was moved forward or backward, it fell against the fore or back part of the tympanum, or else of the barrel itself, but which motion was prevented by placing her fingers and lifting her head in the manner described. These phenomena seem to indicate that the nerves which serve either the tympanum or barrel for the sense of feeling, are so disposed in the sensory or brain, that if the organ be affected in one point, the sensation shall be felt, not as in the part affected, but as in the fore part of the head. If in another part, it shall be felt as in the back part of the head; and perhaps there are other points of that organ which correspond with the whole surface of the head respectively. If this be the case, then, if the air beats upon a certain point of the tympanum, a sound shall be excited in the labyrinth, and at the same time a tremulous sensation of feeling, of the same degree of swiftness, shall be excited in the fore part of the sensory or head, and thereby give us the idea of the sound coming from the front; for the sense of feeling affects us more powerfully, or is more intimate to us

than that of sound; and therefore the attention of the mind is chiefly engaged by the former, and thence translates, as it were, the sound itself thither; or rather, the ideas are associated by custom or habit. To give an instance of this superior power of feeling: though the shapes of objects are painted in the retina, yet it is not merely by these pictures that we get the idea of the shapes of those objects; the eye only considers a point of an object at a time; and it is by the motion of the eye itself, or of the body, tracing its boundaries, that we get the idea of its shape; so that it is done by feeling, not by colour merely, as others have already observed. Every one who has been weakened by a fever will remember how painfully the sense of feeling is excited by strong sounds, and the particular manner in which they affect the head. In general, therefore, if an ærial sound comes from a certain quarter without us, it seems to be made, by the contrivance of the external ear, to beat upon a particular part of the tympanum; the sound, answerable to the time of vibration, is excited in the labyrinth; and at the same time, a like tremulous sensation of feeling as in that
part

part of the head which answers to the situation of the external sound, and this, from custom, gives us the idea of the sound coming from such a quarter. And if two or more sounds come from different quarters without us at the same time, they seem to be made, by the funnel of the ear, to beat upon different parts of the tympanum; and besides the sounds which they excite in the labyrinth, cause respectively unison or like tremulous sensations of feeling, as in the answerable sides of the head; whence we come to understand their situations externally with respect to each other and to ourselves. The tympanum may move as a whole, notwithstanding these vibrations excited in different parts of it; and (by the mediation of the little bones) *according as its motion as a whole is affected by these particular vibrations, the passions or affections of the mind and body may be influenced.* I do not know whether the harmony and discord of sounds, considered musically, may not partly depend on this latter principle.

Obs. IX. That it is by the sense of feeling as above, or, at least, that it is not by mere

found that we get the ideas of the situations of external sounds, seems also to appear by the following considerations. Admitting that different sides of the tympanum are affected, as above, according as sounds come from different quarters, yet the nature of aërial vibrations, and the situation of the tympanum with respect to the passage into the labyrinth, seem to be such that the sounds cannot be directed in right lines from the different parts of the tympanum affected, through that passage, as the rays of light cross in the eye and pass on to the respective parts of the retina, for painting objects answerable to their situations outwardly; and even supposing they could, yet the structures or shapes of the cochlea and labyrinth are such as that no advantage could be derived from this rectilinear direction; they seem to enter into the organs in a confused medley, through every part of their windings, and alike from whatever quarter they come; and it is only the internal sounds, deposited in those organs, that separate them, according as they happen to be in unison. Sound, therefore, seems to have no other concern in the affair of hearing than merely as to
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the note or tone, and the strength or loudness thereof, as observed before.

Obs. X. Distinct hearing seems to be concerned only with a particular part of the tympanum, as distinct vision is only with the middle of the eye. If we would view an object distinctly we turn the optic axis towards it, and even to the several parts of it successively ; and if we would hear any sound to perfection, we turn our face towards the quarter from whence the sound comes ; in such position of the face, the sound falls perhaps on the middle of the tympanum ; or at least on that part of it which serves for distinct hearing. Do the sounds in this case fall upon the malleus ?—or rather, do they not fall in the midway between the malleus, and the margin of the membrane ? At least, do they not beat upon such a part of the tympanum, as that they may be thrown to most advantage into the fenestra ovalis, or else upon the elastic membrane in the rotunda ?

Obs. XI. Distance, with regard to hearing, seems to depend on much the same principles as distance with regard to vision, viz. on the

faintness and indistinctness of the sound beating on the tympanum, &c.

Obs. XII. Though we have two ears, yet we hear singly, even as we see singly with both eyes. Are the nerves for the sense of feeling, which serve the tympanum (or that part of the ear, whatever it be, which gives us the idea of the situation of sounds), joined together in the brain, as the fibres of the retinae of the answerable parts of both eyes are supposed to be? or is each ear concerned only with the answerable side of the sensory? or do we hear only with one ear at a time, as some have supposed that we see only with one eye at a time?

Obs. XIII. May it not be on account of the distance of the labyrinth and cochlea from the outward ear, that the innate sounds are difficultly excitable by external pressure, and that many of them cannot be excited thereby at all? the deepest of these sounds may be in the cochlea, and the highest in the labyrinth; for the high and low sounds seem to be in different parts of the ear. As I cannot excite sounds
lower

lower than G, is it not that only the higher ones in the labyrinth, and not the lower ones in the cochlea, are excitable by outward pressure ?

Obs. XIV. Those who are difficult of hearing, and also such as listen attentively, are apt to open their mouths, and then they hear better. It has been said that in this case the sound passes by the Eustachian tubes ; but whoever chooses to make the experiment will find, by putting a finger into each ear, that when he opens his mouth, the bones of the lower jaw leave the meatus auditorius much wider than when the mouth is shut, which may perhaps be the cause of the phenomenon, at least in part.

Obs. XV. The vibrations in the ear by which the internal sounds are caused, are not of the nature of those of musical strings, for the following reasons : the length of the fibres, especially for the low notes, would be too great for them to be contained in the ear, if they caused sounds by vibrations in the manner of musical strings. If it be objected, that their

tension is proportionally less, I answer, that then by reason of their slackness they could not vibrate, on that principle, at all. *2dly*, There are no fibres in the ear but what are immediately surrounded by such substances as would totally hinder a vibration in the manner of musical strings. *3dly*, There is no trace of any contrivance in the ear, which can in the least favour a supposition of this kind to him who properly considers the above objections; and there are many others which may be urged, but these are I imagine sufficient. In what particular manner these vibrations are performed I cannot determine: it may be by means of fibres whose particles or elements, when irritated, alternately approach towards, and recede from, each other, and thereby lengthen and shorten the fibre by turns, without forming the harmonic curve like a musical string. The fibres for the deeper notes may be composed of larger particles, and thence vibrate more slowly; and on the contrary, the fibres for the higher notes may be composed of smaller particles, and thence vibrate more swiftly; the pulses in both cases being communicated to the nervous fibrils with which they
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are respectively served: the like may be the case in the eye, and the sense of feeling. A single string, or even two particles only, seem proper for this purpose, and they would also take up less room in the organs; and be too minute perhaps to be discovered by the eye. Supposing this to be true, we have a reason why in the experiment of pressing the centre of the eye, the green, blue, and violet-making vibrations are most easily excited, and that their nerves are last paralytic; and contrariwise with the yellow, orange, and red-making ones. The particles of the former being less, are more easily put into vibrations, and less apt to press on the nervous fibrils than those of the latter, which are larger *; and this may also be one reason why the low innate sounds cannot

* By pressing the centre of the eye for some time, a luminous ring of a reddish yellow colour is often perceived; if the pressure be removed, and the eye turned towards the light of the clouds, the ring (which does not disappear for some time) is changed into directly opposite colours, viz. green, blue, and violet; which shews that the pressure had rendered the former internal colours more paralytic than the latter, agreeable to what was said before. M. Le Cat, if I remember right, has experiments to the same purpose, which may therefore be explained by this theory.

be excited by outward pressure, the particles of the fibres, being large, requiring a stronger irritation than can be there applied. But this is conjecture.

Obs. XVI. There are (it may be presumed) many octaves of the internal sounds; but not quite one octave of colours. This difference was requisite, because there may be a great number of vibrations made in the air, which would be lost to us, if there were not answerable ones in the ear; whereas the vibrations of the rays of light being limited to about a sixth, only the like latitude of internal vibrations was required answerable to them.

Obs. XVII. In the eye there seem to be a great number of vibrations which give the same colour dispersed in every part of the retina; and vibrations, in all the different times, seem likewise to be mixed equally together in all parts of that organ. Thus, if any part of the retina be excited by pressure, not a single colour ariseth (unless by accident, as in the instances related in the first section,) but a white one, composed of all the others. But by exciting

ting the innate sounds I can hear many of them distinct ; nor can a sound be excited composed of all the internal ones, as is the case with colours. The reason of this difference is, that the internal colours can be mixed together in the retina, in a space small enough for them to be perceived only as one colour. But this cannot take place with regard to the internal sounds, by reason of their far greater number.

OBS. XVIII. About ten years ago, I observed that a flute, an hautboy, a trumpet, and other instruments, though they were made to yield sounds which were in unison with each other, and equally loud, yet had a difference which every one could observe, and which I then called the *mode* of sound. Thus also the voices of people, and the sounds yielded by various bodies, though of exactly the same tone and strength, had a similar difference. Whether the cause of this curious phenomenon had been discovered, I could not learn ; but by meditating on the subject, and making several experiments, I found that these sounds were not simple, but composed of others, of which these were only the result or aggregate, even as the colours

colours of bodies are various compounds of the several original colours. I am told, by a gentleman to whom I communicated this theory, that a discovery of this kind is already made public, though I have not yet been able to get a sight of it : I shall not give the observations which suggested that theory to me, lest they should be similar to those alluded to *. But as I cannot find that an explanation has been

* The principle on which these sounds were capable of being decomposed was, that in many cases some of the sounds in the mixture were stronger, and others weaker: hence if they were excited as gently as possible, or rather if I removed the cause of them to a sufficient distance from me, the weak sounds in the compound would be inaudible, and only the stronger ones heard. Suppose a sound composed of C D and E, that C was double the loudness of D, and D double that of E; the tone of that sound would be in a compound ratio of the tones and strengths of the ingredients; that is, it would be a sharp C. If E be rendered inaudible, the sound, as being composed only of C and D, would be nearer to C; but if D also be made inaudible, the sound would be pure C. If the ingredients are equal in strength or loudness, this decomposition cannot be made. This theory was suggested to me by like observations with regard to colours; for some objects, according as they are more or less strongly illuminated, appear differently coloured, for reasons (as I imagined) similar to those given above.

given

given of this phenomenon with regard to what happens in the ear, I shall here subjoin a conjecture concerning it: it was shewn above, that all the internal colours may be excited within a space of the retina small enough for them to be perceived only as one colour, the aggregate of the whole. If aërial sounds are blended together, as in the cases just mentioned, they are excited so near to each other in the tympanum, that they are heard only as one sound, as happens with a mixture of colours; but if the sounds are excited at a distance from each other in the air and tympanum, they are heard distinct, as happens when, in vision, colours are painted in different parts of the retina. But to enter a little closer into this reasoning, those who are versed in optics know, that if any two neighbouring colours in the refracted spectrum be mixed together, the colour arising therefrom will be such an one as would be caused by the rays in the mean betwixt them: thus, if blue and yellow be mixed together in equal quantities, the colour will be green; and if the quantities be unequal, the green will be tinged with yellow or blue in proportion; the like may be
observed

observed of other neighbouring colours. But if red and violet be mixed together, the colour will not be a green, or the intermediate one, but a kind of purple, unlike to any of the original colours. Also, if any number of colours are mixed together, provided the two extreme ones are at a sufficient distance from each other in the spectrum, there will not be produced the intermediate prismatic colour, but some one unlike to any of these : thus, a mixture of all the rays compose a white, and so of other mixtures; for further information in which, Sir Isaac Newton's Optics may be consulted. Now, I would suppose that a single series of colorific vibrations in the retina are disposed in a right line according to their times, as in a refracted beam of light, and that this line exceeds the diametre of a visible point, yet is not so long as that the two ends of it may be perceived distinct. Hence the red and violet only, though they are not separately distinguishable, yet as they do not fall within a visible point, they also cannot be perceived as a perfect mixture or under the form of the intermediate colour; they must therefore be perceived as in a state between perfect mixture, and distinctness; and

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we find that a purple is the result, a colour in which the ingredients can in some measure be inferred by the eye. But two colours which are near each other, are contained within a visible point, and therefore may be said to be mixed intimately together, for they exhibit the proper intermediate colour, as was shewn above. For the same reasons, all the colours in the series together ought not to exhibit, like yellow and blue, the intermediate colour, nor any of the original ones, because of the red, violet, &c. which are exterior to the visible point; neither ought the colour exhibited to be such an one as that the ingredients may in a manner be inferred by the eye, as is the case with red and violet alone, because the whole series is a composition of perfect and imperfect mixture; and we find that they compose a white *. Now if we apply these principles

* If we suppose a number of these series joined together in a right line by their answerable ends, viz. red to red, and violet to violet, that the whole surface of the retina is filled with such lines drawn parallel to each other, and that these lines are crossed, at the red and violet points, at right angles by similar ones respectively; the whole surface will be divided

ciples to the ear, and consider hearing as composed of sound, and a tremulous sense of feeling, as mentioned before, we may be enabled to form some idea of the cause of the above-remarked difference of sounds, whose tones and strengths are the same. The innate sounds are not, like colours, comprized within so small a portion of the ear as that they may be all heard as one: on the contrary, experiment seems to shew that they are *distinct*. The above doctrine, therefore, is not applicable to the innate sounds; it must of course be applied to that part of hearing which depends on the tremulous sense of *feeling*, and by which the other is governed, as hath been shewn. If two or more ærial vibrations fall within the same point of the tympanum, they may be considered as mixed perfectly together *; and therefore
a sound

ded into squares, the sides of which will be less than the distances required for distinct vision.—But this is a mere hypothesis.

* Query. Whether in any instance of this kind, two or more vibrations are converted, as it were, into one?—For example, whether a sound and its octave make, not the intermediate sound, but the octave? I do not think this ever to be the case; but that they continue distinct, and therefore
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a sound will be caused whose note is the mean of all these, and whose mode is the same with that which would be produced by a single vibration. But sounds in the tympanum further apart, yet not so distant as to be heard distinct, though they yield a sound of the intermediate tone, yet the mode thereof, by reason of the imperfect mixture, shall be different. And if the sounds are excited at a still greater distance, they shall be heard distinct; and therefore by assuming the hypothesis of the tremulous sense of feeling, and carrying with us the idea of perfect mixture, of indistinction or imperfect mixture, and of distinct sounds, as above, duly combining these, varying their strengths, and taking into consideration what happens in the cochlea and labyrinth with regard to the innate sounds, and also the passions or affections of the mind, we may have perhaps a theory of this kind of phenomena. But as this is a subject which does not easily admit of experimental proof I shall not enlarge on it.

the intermediate tone is the result. The reason why some modes are more pleasing than others, may perhaps be collected from the eighth Observation.

SECTION V.

Being an Appendix to the foregoing Essay.

IT may not be improper to acquaint the Reader, that my situation in life has hitherto been such as to have afforded me but few opportunities of reading books which were not in the line of my profession; many discoveries in philosophy therefore had been made public which I was unacquainted with: many I suppose still remain, of which I have no idea. As I am fond of amusing myself at my leisure with studies of a philosophical nature; ideas have occurred to me which I thought were new, but which I have afterwards found in authors. The doctrine of the modes of sound occurred to me above ten years ago, (as indeed did almost the whole of the preceding essay). I mentioned that I had since shewn it to a friend, who informed me that the discovery had already been published by Tartini, an Italian.

Italian. But since that essay was sent to the press, I happened to meet with a translation of Rousseau's Musical Dictionary, and find by it that the theory in question is not yet known, Tartini's discovery being of another kind, viz. *The harmonic sounds which arise in consequence of any musical sound*; and on which he has founded a new system of music.

M. ROUSSEAU, after explaining the two differences of sounds, the *tone* or *note*, and the *strength* or *loudness*, speaks of this third difference of sounds; and expresseth himself as follows :

“ IN regard to the difference which is found also between the sounds by the quality of the modification, it is evident that it holds neither to the degree of elevation, nor even to that of the force. It will be in vain for an hautboy to place itself in unison with a flute : it will be in vain to sweeten the sound to the same degree ; the sound of the flute will always have a *Jc ne sais quoi* of mellow and sweet ; that of the hautboy somewhat rude and sharp, which will prevent the ear from confounding

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them,

them, without mentioning the diversity of the modification of the voice.

“ THERE is not an instrument which has not its particular tone which has no connection with that of another ; and the organ alone has twenty methods of playing, all of a different modification. *No one however that I know of has examined the sound in this particular*, which, as well as the rest, will perhaps be found to have some difficulties ; for the quality of the modification cannot depend either from the number of vibrations which forms the degree from flat to sharp, or from the greatness or force of these same vibrations which forms the degree from strong to weak : we must then find in the sonorous body, a third different cause from these two, to explain this third quality of sound and its differences, which perhaps is not too easy.” Thus far Mr. Rousseau.

I SHALL therefore give the Reader the observations from which I was afterwards led to suspect this theory, and in the very words of the original paper which I have long had by me.

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“ ————THE innate sounds are the most simple possible, as they are made (we may suppose) only by single or simple vibrations; whereas the aërial sounds which we hear, are all of them more or less compounded. And on this composition of them, the solution of some curious phenomena seem to depend, which have not been attended to by the learned.

“FOR besides that sounds are higher or lower in tone according to the swiftness of the vibrations which cause them, there is also another property in which sounds differ from one another, though of the same strength and tone: an organ, for example, has several kinds of stops; there is one which being played resembles a flute; another which gives the same musical notes as the above, yet differ very much in sound: for whereas those resembled a flute, these sound like a trumpet; another stop gives the same musical notes as both these, yet resemble an hautboy. The like may be instanced in bells, the string of an harpsichord, violin, &c. So I can with my mouth sound a row of notes, or sing a song in a manner resembling a flute; I can sound the same notes like a trum-

pet, an hautboy, and other instruments: so in conversation, I can talk in a great many different voices, though with the same tones and loudness. There are hardly two people in the world whose voices are exactly alike, though they were to talk in the same musical tones, and equally loud; and it is from hence that we know the voices of people who are talking to us. The sounds also which are yielded by bodies that are struck are different, though of the same tones and strength, insomuch that there are hardly two kinds of bodies which sound exactly alike. This difference of sounds I call the *mode* of sound to distinguish it from the *tone* and *strength*.

“OBSERVATION I. If I stand by a large church bell when it is struck, and listen attentively to the sound when it is almost vanished, I can distinguish not one sound or note only, but several; for the real note of the bell will go off and be no longer heard, and instead thereof other sounds different in *tone*, as well in *mode*, will arise, which in some bells are more and in others less in number: and different
bells

bells exhibit this phenomenon with different degrees of distinctness.

“ O B S. II. I lived near a church in which were eight bells, and the clock struck on the the first four of them every quarter of an hour, I have frequently observed that if I was so situated as that the sounds could hardly be heard, or heard indistinctly, the sounds of these bells, which otherwise were D, C sharp, B, A, in these cases totally lost their succession; they gave quite different tones, as G, E, C, B, or others, and their *modes* likewise were different.

“ O B S. III. I have observed the like in the voices of people under similar circumstances, and also in the sounds produced by various other methods. I mean in cases where the sounds were hardly sensible, or indistinctly heard.

“ O B S. IV. I once saw a peal of small house bells, with which a young gentleman used to amuse himself; if I stood near these bells when he rang them they were very tunable, and made good music; but if I removed to a distance from them, though I heard the bells

distinctly struck, they no longer yielded sounds in succession, every one a note lower than the preceding, but quite irregular and confused: the irregularity was different at different distances, and the *modes* were altered as well as the tones.

“ FROM these observations I gather *that the tones of sounds which are yielded by bells, &c. are not simple, but composed of other tones.* Thus in optics, the rays of light in the fourth series of a spectrum cause a green colour, which being produced by one sort of rays only, may be termed simple; the rays of the third series are yellow, and of the fifth blue: yet when mixed equally together, they no longer appear as yellow and blue; but a colour results from their mixture, which is the same as would be caused by the simple rays of the fourth series. If these rays are again separated, they no longer cause a green, but their proper colours; if the rays are mixed in unequal quantities they cause a green, not like the other, but inclining to the colour of the greatest quantity of rays. Also, if either in equal or unequal quantities they are mixed not perfectly together (so that
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for example, if the object from which they come be viewed near, or but faintly illuminated, these colours may be seen distinct; yet if viewed at a distance, or the object be strongly illuminated) they cause an imperfect green, so all the seven original colours mixed together produce a white, and the like.

“BODIES in general do not appear exactly of the same colours when viewed by a strong, as when viewed by a faint light, which I take to arise from hence; that bodies do not reflect an equal quantity of each sort of rays. Suppose that a body reflected four parts of red, three of orange, two of green, and one blue; if the light be so faint as that the red rays chiefly be sensible, the object will be redder; and if the light be still stronger, the colour will vary from that red with the increase of the illumination, till it appears of that colour which ought to result from the above mixture,

“FOR the same reasons, if two sounds are mixed equally together, and of equal strengths, but of different tones, they will cause a sound whose tone is the mean between the tones of the two sounds when separate: thus G and B
being

being mixed, the note will be A ; if they are unequal in strength, the tone will incline towards G, or B in proportion ; and all the other instances of colours above given may be applied in some measure to sounds.

“ So then the reason of the change of tones of sound by distance, inattention, or the like, ariseth from hence ; that if the sounds which are blended together be of unequal strengths, those which are strongest must reach to a greater distance than those which are weaker ; so that the weaker ones not affecting the ear, or not with sufficient force, only the stronger sound or sounds which reach the ear will be perceived ; but if the sounds are equally strong, this will not take place. The sounds of the bell in the first observation, however, are to be understood by what was said above of colours being mixed imperfectly together, and therefore they were not heard as one sound but when they were sufficiently strong, so as to spread their effects on the sense into one another *.

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* The note of latitude, as it is called, in wind instruments, may depend on the principle of the mixture of sounds of different

“IT has usually been thought that all sounds affect the same parts of the ear; but the fact appears to be otherwise. And it is surprizing how near sounds may be to each other in the air, and yet be heard distinct, and even when no longer heard distinct, they are preserved separate: it does not appear that two sounds form only one vibration when thus mixed, but the intermediate tone. A theory of aërial sounds in this view therefore is yet wanting, as well as that of different sounds yielded at the same time by the same body.

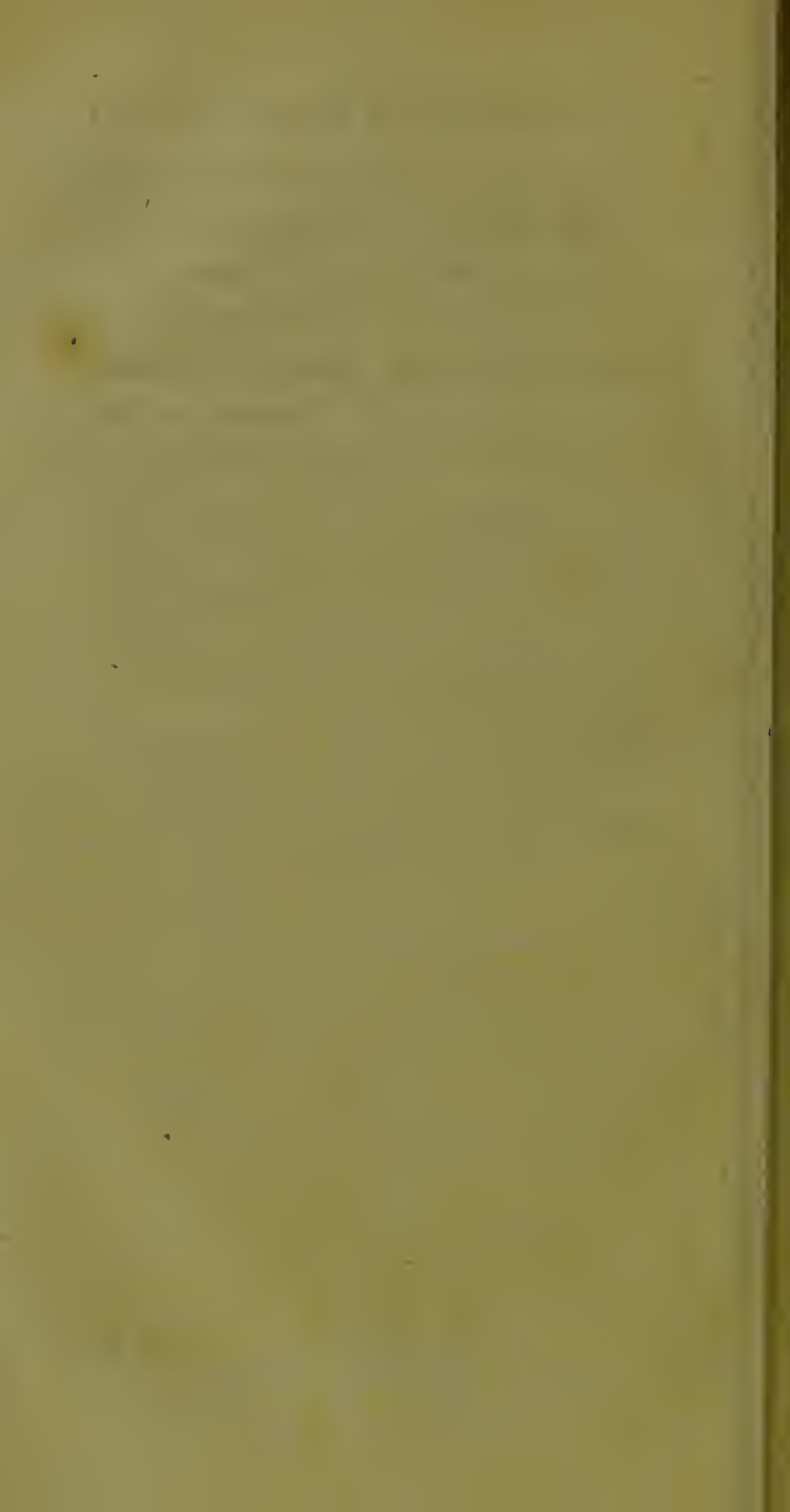
“IF the sounds thus mixed together are concords, they form perhaps the sweetest modes; and on the contrary, the disagreeable modes seem to be composed of discords. The last five or six of Bow bells are, I think, the most agreeable in peal of any that I ever heard; and the reason is that the founder has judiciously varied the *modes* as well as the *tones*. This observation might probably be applied to good use by lovers of music.

Now these complex sounds, though we perceive different strengths, all of which are not audible but by a strong blast.

ceive them as one, do not, I take it, excite that innate sound in the ear which answers to the tone we hear; but every one of the vibrations which compose that complex sound excites its unison, which are so mixed together in the sense that we perceive only one sound, the result of the whole, as before observed.—”

WHAT relates to the sense of feeling (of which I had no idea at the time of writing the above) has already been explained, and which seems to have a great share in these modes, inasmuch that the mixture of the sounds, and their being heard as one, depends on it, and not on the innate sounds, as hath been shewn. The innate sounds may be excited in greater or less number; stronger or weaker; and in more or less harmonic or discordant relation to each other, which seems to be all that sound contributes to these modes. The tremulous sense of feeling excited in the tympanum, the parts connected with the little bones, the portio dura, the nerve that adheres to the tympanum, the various distributions of the twigs of the nerves of the ear, so as to form sympathies with other parts, and the association of ideas
are,

are, perhaps, all concerned in these modes, though chiefly the first. I have only begun the subject, and would wish to see it further prosecuted by those who have leisure and inclination. In the mean time it may, I think, be admitted, that “as the colours of bodies are not simple, but made up of others, according to the different mixtures of the rays of light issuing from them, so neither are their sounds simple, but composed of several or many others, which the body by its various vibration emits; and which, like the colours, are so mixed together in the sense, as to appear but one, the mean of all the ingredients. The *modes* of the sounds depend on the manner of this mixture.”



A
T R E A T I S E
O N
HARMONIC SOUNDS.

INTRODUCTION.

AS I have had occasion to mention Music in several places of the preceding Essay, the following may be inserted with some degree of propriety.

SOME time ago I found out the theory of the harmonic sounds yielded by musical strings, and constructed a scale of them for four stringed instruments tuned fifths, without then knowing that they had been published long before. I have since been undeceived by the friend who informed me that Tartini had discovered the theory of the modes, and by Mr. Rousseau's Dictionary. There are two very material points, however, with respect to the practice of these sounds, which I cannot find any account of in that Dictionary; and therefore, as my paper on this subject is short, I will subjoin the whole of it.

THE

THE
THEORY
OF
HARMONIC SOUNDS.

THE *common* sound of a musical string is caused by a simple or single vibration thereof; the *harmonic* sound by a various vibration;

IF one of the strings of a violin be struck with the bow open, it vibrates, and thereby yields a sound: if the finger press the string in the middle upon the finger board, its lower part only will vibrate, and its vibration will be twice as swift as that of the whole string, so that the sound will be an octave above the former.

BUT if the finger be laid lightly on the string, without pressing it on the finger board,
both

both halves of it will vibrate; their vibrations will coincide with each other; the sound arising therefrom will be much sweeter than in the other case, and will be the harmonic octave to the open string

If the finger be placed at one third of the string from the nut, and struck with the bow, it will vibrate in three distinct and equal parts, coincident with each other. You may prove this by laying another finger lightly at two thirds; for though the string be thus stopt double, the sound will be the same; though if you remove either or both of the fingers from these points, either higher or lower on the string, the note ceaseth. Also, if with a finger at $\frac{1}{3}$, you bow at $\frac{2}{3}$, no such sound will be excited; but if you remove the bow sufficiently from this point either way, the sound again ariseth *. The vibration in this case being to
that

* You may see this threefold vibration, at least in the silver string: but in a bass viol you may see it much plainer, and there the four, five, and sixfold vibrations, may also be distinguished by the eye. Each harmonic stop is not confined to the point, but has a latitude, and the points of the $\frac{1}{2}$ and $\frac{2}{3}$ divisions have a greater latitude than the lesser divisions. The

that made by stopping at one half, as 3 to 2 makes a fifth above it.

PLACE the finger at one fourth of the string from the nut, it will vibrate in four distinct and equal portions, coincident with each other, and being twice as swift as the first, the sound will be an octave above it.

REMOVE the finger successively to $\frac{1}{5}$, $\frac{1}{6}$, $\frac{1}{7}$, $\frac{1}{8}$, $\frac{1}{9}$, of the string, it will vibrate in 5, 6, 7, 8, and 9 equal parts; and you will, for reasons similar to those given above, have a sharp third, a fifth, and very sharp sixth (or flat seventh) to the octave; an octave to that, and a full tone above that double octave, and you may go still higher, by taking the $\frac{1}{10}$, $\frac{1}{11}$, $\frac{1}{12}$, &c. of the string.

THE above notes may be made by stopping at any other point besides $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, * &c. and like-

reason of this latitude is, that near the points the motion of the string is very small, and therefore is not much interrupted by the finger; whereas, nearer the middle, the vibration is more easily stopt. The tone is also a little altered when the finger is not exactly on the point; the reason is obvious.

* Any of these frets may be used in practice, when more convenient than the other.

wife by stopping at all, or more than one of these divisions: thus, the fifth above the key may be made by stopping at $\frac{2}{3}$ from the nut, as well as at $\frac{1}{3}$, and also by stopping at both $\frac{1}{3}$ and $\frac{2}{3}$. The octave may be made by stopping at $\frac{1}{2}$ or $\frac{3}{4}$ as well as at $\frac{1}{4}$ from the nut; and also by stopping at all, or more than one of these points (which may be done by threads fastened across the string round the instrument). The sharp third above the octave may be made by stopping at $\frac{2}{5}$, $\frac{3}{5}$, $\frac{4}{5}$, as well as at $\frac{1}{5}$, and likewise by stopping at all, or more than one of these points; and so of the rest. Whence you have this caution; “that if you do not happen to stop right, some other note than that intended may arise.” Thus, if you place your finger a little below $\frac{1}{3}$ from the nut, you light on one of the $\frac{1}{3}$ divisions, and so have, instead of a fifth, the sharp tenth: a little lower you fall on one of the $\frac{1}{7}$ divisions, which gives the sharp 13 (or flat 14) and so of others; of which therefore you must be aware.

ALSO you must be careful not to bow upon the points or divisions of the strings, for then either no sound will arise, or not that designed,

but between these points: thus, if you stop at $\frac{1}{4}$ you must not bow at $\frac{3}{4}$, but between that and the bridge, or between other points, though that next the bridge is best, the string being most steady there. From whence also it appears "that you must bow nearer to the bridge in proportion as you use an higher fret," the division being less.

LIKEWISE when you stop at $\frac{1}{2}$, you will, instead of the key, often get its octave, unless you bow towards the verge of $\frac{3}{4}$ from the nut, because the $\frac{1}{2}$ fret is also a $\frac{1}{4}$ division *. But by bowing near to $\frac{3}{4}$ you do not excite the octave, for reasons which may be seen above; and so of other notes.

A STRING is so apt to run into harmonic vibrations, that these sounds may be raised merely by bowing on proper parts of it, without stopping with the finger: thus, if you bow on the proper parts of the silver string near the bridge, you have thirds, fifths, eighths,

* It is likewise a $\frac{1}{6}$, $\frac{1}{8}$, $\frac{1}{10}$, &c. division. The like may be observed of the $\frac{1}{3}$, and other frets: and the various respective sounds may be raised by bowing properly, as above.

and

and other harmonic notes ; and they may likewise be raised by bowing on other parts of the string, by observing what was said in the preceding paragraph. The bow, in these cases, acts in a double capacity, for it both stops and vibrates the string *.

FROM what has been said, it appears “ that the harmonic sounds are made by stopping the string lightly, according to the proportions in

* I find by the Dictionary of Music, that Tartini has founded his system on this observation : I am mistaken, however, if he has not proceeded on a wrong principle. He says (if I remember right, for I have not the book now by me), that when a string is sounded, all the notes harmonic to the sound naturally arise with it ; and he applies it to all other sounds. It is true that if you strike a string with a bow, you will often raise some of the harmonic sounds, for a reason given in the last paragraph ; and that a string should vibrate as a whole, and in distinct parts at the same time, is as easy to conceive as that the moon can revolve at once round the sun and our earth. If, however, you excite the sound of a string by any other means, as by striking it with a stick, or pulling it with the finger, I do not find that any such sounds arise. I have not yet had leisure to satisfy myself concerning this matter ; but mean to examine both this, and what Tartini says of the *third sounds*, when I have a convenient opportunity.

the following series, viz. $\frac{1}{2}, \frac{1}{3}, \frac{1}{4}, \frac{1}{5}, \frac{1}{6}, \frac{1}{7}, \frac{1}{8}, \frac{1}{9}$, &c. the string vibrating in as many distinct and equal portions as the denominator hath units, all in unison with each other; and the sounds being higher according as the portions of the string become shorter; that is, according to the swiftness with which those parts vibrate." This may suffice for the theory of these sounds, we may now proceed to

THE PRACTICE.

THE strings of the violin, &c. being tuned fifths *, the harmonic notes on them will be as in the following scale.

* By means of these sounds the instruments may be tuned to the greatest exactness: to do which you have only to screw up the strings so as to bring the 5, 9, 13 on the line $\frac{1}{2}$ in unison with the 5, 9, 13 in the line $\frac{1}{3}$ (see the scale); and as the ear can better judge of an unison than a fifth, you may tune to greater perfection than in the common way. This also I could not find in Mr. Rousseau's Dictionary.

SCALE.

S C A L E.											
Fourth String	C	1	G	5	C	8	F	12	B ^b	15	&c.
Third String	G	5	C	9	F	12	B ^b	16	C	19	&c.
Second String	D	9	A	13	D	16	F	20	B	23	&c.
First String	A	13	E	17	A	20	C	24	F	27	&c.
The divisions from the nut	$\left\{ \begin{array}{ccccccc} \frac{1}{2} & \frac{1}{3} & \frac{1}{4} & \frac{1}{5} & \frac{1}{6} & \frac{1}{7} & \frac{1}{8} & \text{\&c.} \end{array} \right.$										

You may carry it still higher, by adding frets above $\frac{1}{8}$; but this commands a sufficient compass of notes for practice. It has, however, the inconvenience of being incomplete, especially in the lowest and best notes.

WITH a view therefore to improve this scale, or obtain one more perfect, imagine the strings to become continually shorter, or that the bridge and nut approach toward each other with a regular motion; the divisions of $\frac{1}{2}, \frac{1}{3}, \frac{1}{4}, \frac{1}{5}, \frac{1}{6}, \text{\&c.}$ will still remain in the same proportions, but on a scale continually contracting or lessening; and the sounds will become higher in proportion to the shortness of the strings.

To reduce this to practice, place the little finger lightly on $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, or any other harmonic fret of either of the strings; place the fore finger on the nut, and strike the harmonic sound with the bow, continue the bowing while you slide your fore finger from off the nut upon the string, pressing it down hard on the finger-board, and from thence along the string up towards the bridge, bringing your little finger nearer towards it, so as that you may be always at $\frac{1}{3}$ (if you use that fret) of that part of the string between the fore finger and bridge, so shall you have a continually ascending harmonic sound. From whence it appears, “that you may make an harmonic sound of what degree of sharpness or flatness you please” (within the compass of the instrument): with sounds made after this manner, therefore, you may supply the deficiencies of the above scale, at least from 5, upwards; whereby you may make it as perfect as you please.

OR you may compose your scale intirely on this plan, (though it must be owned that the notes are less harmonious than when the strings are not hard stopt); thus, make G, A, B, C, on the

the silver string as in the common way, stopping hard for those notes with the fore finger, and making them harmonic by stopping lightly with the little finger at $\frac{1}{3}$ of that part of the string between the fore finger and bridge, and so on with the other strings. By this means you have a compass of sixteen notes, using only one fret, and going no higher than A on the treble string. But by shifting the hard stop to B, C, D, &c. you may go still higher; and higher after all by changing the fret for those above. You may also shift on the other strings, and on any part of any string you may by this means make not one only, but as many harmonic sounds as your fingers can command frets. The practice indeed is somewhat difficult, but can be done, I imagine, sufficiently well by one used to shifting and double stops; or, in other words, by a master of the violin.

BUT the best scale for practice that has yet occurred to me is the following.

A SCALE of the harmonic notes of the violin, according to the Diatonic Genus; which therefore might easily be varied for the other genera, and also for other instruments of the the viol kind.

Take only the following notes of the former Scale.

G	D	A	E	$\frac{1}{2}$
*	*	*	*	$\frac{1}{3}$
*	*	*	B	$\frac{1}{4}$
B	F ^{sh.}	C ^{sh.}	G ^{sh.}	$\frac{1}{5}$
G	D	A	E	$\frac{1}{6}$
D	A	E	B	$\frac{1}{7}$
G	*	*	*	$\frac{1}{8}$

These notes run thus : G, *, *, *, D, *, *, G, A, B, *, D, E, Fsharp, G, A, B, Csharp, D, E, *, G, A, B, *, *, E.

I HAVE rejected the D, A, E in the line $\frac{1}{2}$, and also those in the line $\frac{1}{6}$, because they are more convenient for playing in the lines $\frac{1}{3}$ and $\frac{1}{4}$; I reject all the notes above $\frac{1}{8}$, and likewise those

those in $\frac{1}{7}$, because they are too difficult to hit, and because those made by the hard stop are more harmonious. Perhaps those on $\frac{1}{8}$ might also be rejected for the same reason. The vacancies may be filled up as follows.

THE fingers not being long enough to complete the notes from G to D, the scale can only be perfected from D upwards.

IN order to this, place your fore finger on the silver string, as for making the common G sharp, pressing the string down on the finger-board, as in the common way, at the same time lay your little finger as lightly as possible on the string at one third part of the distance between your fore finger and the bridge, sound with the bow, and you have the harmonic D sharp.

REMOVE the fore finger, as for making the common A, and the little finger a little farther on, you have E.

MAKE common A sharp with your fore finger, and remove your little finger somewhat nearer to the bridge, you have F.

MAKE

MAKE the common B with your fore finger, and remove your little finger a little farther on, you have F sharp.

MAKE the common sharp C with your fore finger, and place your little finger at one third part of the distance between your fore finger and the bridge, you have G sharp.

A sharp, C, C sharp, Dsharp may be made on the third string in the same manner as Dsharp, F, F sharp, G sharp were made upon the fourth; and in a similar manner you may proceed to fill up the vacancies in the remaining part of the scale, the particular directions for which would be needless, after having explained so fully thus far. *The $\frac{1}{2}$ stop always making the harmonic octave fifth to the note made by the fore finger in the common way.*

IT is somewhat difficult for those whose fingers are short, to command the $\frac{1}{2}$ stop to advantage. In that case, the $\frac{1}{4}$ stop may be used as follows; but the scale can only be completed from the second G upwards.

MAKE common G sharp on the silver string with your fore finger, and at the same time lay your little finger as lightly as possible on the the string at $\frac{1}{4}$ of the distance between your fore finger and the bridge, you have the harmonic G sharp.

MAKE the common A sharp, C, and C sharp with your fore finger, and place your little finger at one fourth of the distance between your fore finger and the bridge as lightly as you can, you have the harmonic A sharp, C, and C sharp. And in the same manner you may proceed with the other strings. For observe, that *whatever note you make with your fore finger in the common way, by laying a finger lightly on the string at one fourth of the distance between your fore finger and the bridge, you make the harmonic double octave to that note;* which rule is perfectly plain and easy for practice. The sounds, however, are not quite so fine as those made by stopping at $\frac{1}{3}$; and in neither case are they so fine as when made by the open string, without the use of the fore finger, except in the instances mentioned before; for
which

which reason the notes in the scale above should be used whenever they can.

N.B. If the string be pressed down, not with the fleshy part of the fore finger, but with the nail, the sounds will be much better. Also *in general* if, when you have struck a note, the finger which makes the light stop be taken off from the string together with the bow, the sound will continue a while after, and therefore be more pleasing; in the open string especially this has a fine effect when properly executed. But if this rule be observed only when you use the hard stop, and not when you use the open string, the sounds will be brought to an equality of sweetness; at least a good performer will be able to do it so well, that the difference shall not be sensible to an ordinary ear. The rule, however, may be observed to great advantage in the following scheme, where only open strings are used. (*That scheme, and the perfecting of the former one by means of the hard or fore finger stop, are the two particulars which I could not find in Rousseau, as mentioned before.*)

A DE-

A DESCRIPTION of an HARMONIC VIOL; the Scheme of which may be applied to any instrument of the viol kind.

THE SCHEME.

Fourth String	1	5	8	10 ^{Sh.}	12	14 ^{Fl.}	15	&c.
Third String	2	6	9	11 ^{Sh.}	13	15 ^{Fl.}	16	&c.
Second String	3	7	10	12 ^{Sh.}	14	16 ^{Fl.}	17	&c.
First String	4	8	11	13 ^{Sh.}	15	17 ^{Fl.}	18	&c.
Divisions from the nut	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{1}{4}$	$\frac{1}{5}$	$\frac{1}{6}$	$\frac{1}{7}$	$\frac{1}{8}$	&c.

WHEN you are playing the violin, or other viol in the common way, and would introduce at times the harmonic notes, you must do it according to the directions already given. But for playing a piece all through in harmonics, you may use the above scheme, *the strings of the instrument being tuned each one note above another*. The notes will then lie in a very natural and easy order for playing; and the strings being open, you may manage these sounds to the greatest advantage. You may tune it to any instrument or pitch at pleasure; and you may also flatten or sharpen any of the strings answerable to the key, only remembering that
all

all the notes on these strings are then flat or sharp; and as most of the notes are double, you cannot be at a loss for the natural ones, &c. Imagination also may make this scheme still more complete: if, for example, you pitch in 8, you may sharpen the second string, if a sharp key, and suppose the notes on $\frac{1}{2}$ and $\frac{1}{7}$ out of the question, so that the notes you want will run on in a more easy and natural order, and the sharp string will also give the sharp thirds and sevenths all through. The like of other keys or pitches. In some pitches you may take only three strings, and tune the other a fourth, fifth, eighth, a flat, or sharp, or whatever you have occasion for. Thus, if you pitch in 6, you may tune the fourth string a fourth under, by which means you not only have that fourth more convenient than by going down to $\frac{1}{2}$, but have also the octave below the key, with other notes above, which some performances might require: or you may add a fifth, or sixth string, and reserve them as by-strings, for these and the like purposes.

ONE finger can very easily manage the notes on each fret or cross line, as the strings are not

to be pressed down, but the finger flipt as lightly over them as possible. The strings should be all of a size, or nearly so; not small; and as even and clear toned as possible. If the instrument was longer than a violin (I mean on account of the strings), and if it was made somewhat like a mandoline or guittar, perhaps the sounds would be more melodious; such an instrument would do very well to play harmonics all through with; and a master would not be at a loss to play with it by turns (by means of shifting) in the common way also.

P. S. SINCE the note in page 69 was sent to the Printer, I have satisfied myself that the harmonic sounds which arise by bowing, depend entirely on the bow, as therein observed; for.

I. No such sounds ever arise by making the string sound by any other method that i can discover,

II. THE sounds which arise depend entirely on the part of the string bowed upon: and the

F

part

part of the string to be bowed on, in order to produce any given harmonic sound, may even be *calculated*, by proceeding on the data delivered in the second and third cautions in the theory above.

III. THE bow therefore acts in a double capacity in these cases, both stopping, and vibrating the string, as before observed.

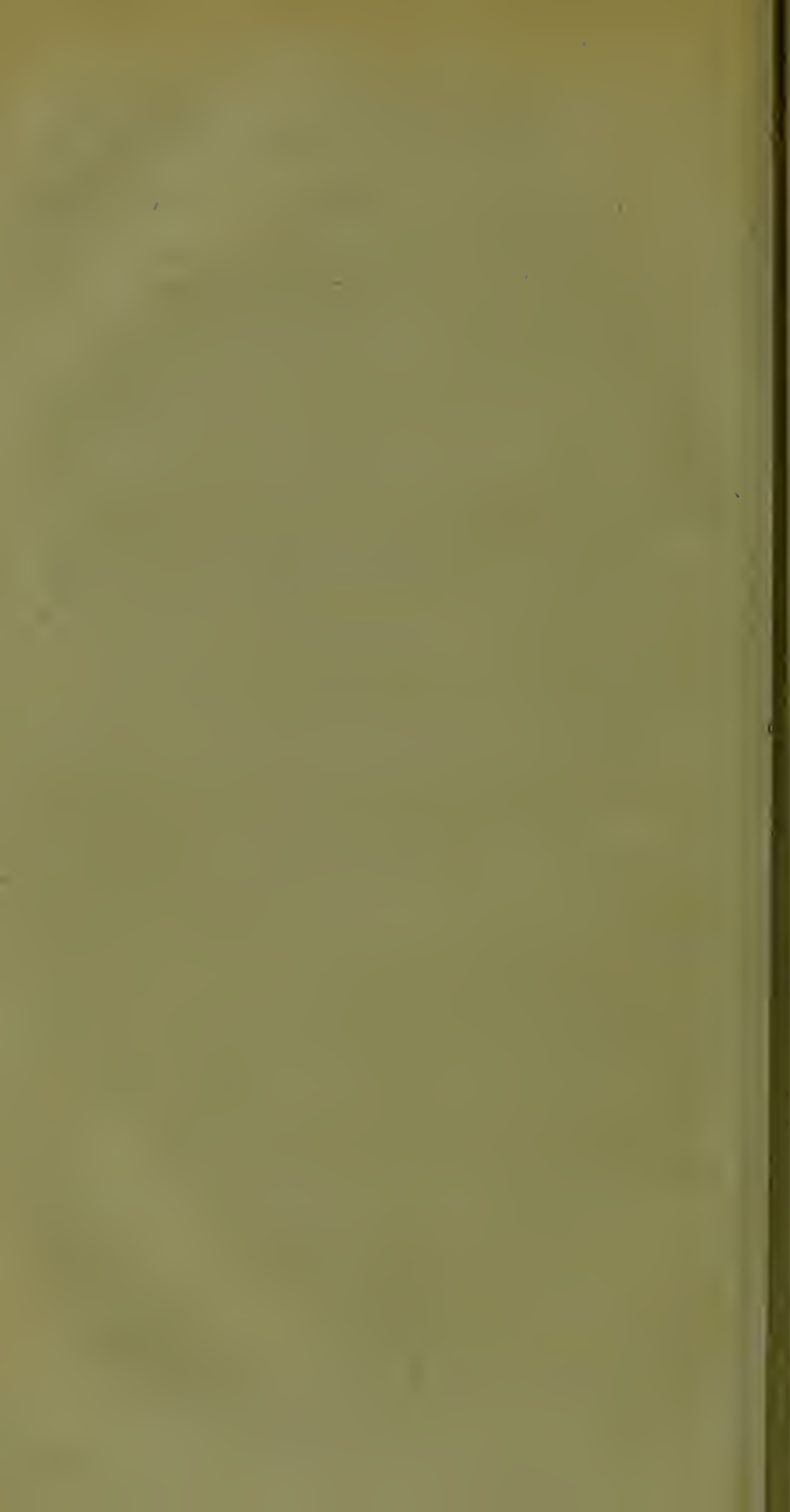
IV. THE eye can very easily distinguish when a string vibrates harmonically, and when only in the common way; in the latter case, the whole string freely and visibly forms the harmonic curve; in the former only its aliquot parts. Both these cases may indeed happen together, as hath already been noticed, but then the latitude of the vibration of the whole string is proportionally and even visibly affected.

V. IF Tartini's theory were just, the stronger the string was made to vibrate, the louder would the harmonic sounds be excited: but the contrary of this obtains; for in order to raise these sounds we must bow very lightly, for if the bowing be strong no such sounds are heard.

I COULD

I COULD enter into a more ample refutation of this theory; but these few hints will be sufficient to the philosopher and mathematician. If any one chooses to examine the matter by experiment, he will do well to observe that bowing with a common bow, and with a single hair makes a very material difference; the former occupies a greater space on the string, and therefore raises more notes, and in a more irregular manner: but this is avoided by using only a single hair.

I WOULD wish, however, not to be misunderstood. That a string of an harpichord, &c. when sounded affects all those strings that are concords, I by no means deny; they do it on the same principle that one string excites another which is in unison with it, and which is too well known to philosophers to need explanation. But that a string when sounded raises also the various harmonic sounds which that string yields by vibrations in its aliquot parts, is, I think, sufficiently refuted by what has been said, or at least could be refuted by pursuing these hints.



A N
I N Q U I R Y
C O N C E R N I N G
C O M B U S T I O N.

S E C T I O N I.

The principal Phenomena of incombustible Bodies.

I. **I**F an incombustible body be exposed to the focus of the sun's light collected by a burning glass, to a culinary fire, to friction, or the like, it will become hot; and its heat will be greater, according to the power of the agent. The heat will continue as long as the cause continues to act.

II. **B**UT if that cause be removed, the body loses its heat by degrees, till it becomes of an equal temperature with the substances around.

III. BODIES are expanded by heat, and contracted by cold; and different bodies in a greater or less degree, according to their density, the cohesion of their particles, and other circumstances.

IV. IF a solid incombustible body be heated, and another be applied to it cold, the former will communicate heat to the latter, and if the heat of the former be sufficiently kept up, it will in the end cause the latter to be hot to any possible degree.

N.B. By *cold*, I mean a degree of heat less than that of the common temperature; and by *heat*, the contrary. But it is more philosophical to use only the term *heat*, and to consider bodies as more or less hot according as they raise or sink the fluid in the thermometer.

V. WHEN an incombustible body is heated to a proper degree, it emits light, so as to cause the body to appear luminous to the eye, the light increases with the heat; but if it be suffered to cool, the light decreases again with the heat; and when it arrives at
about

about the same degree as when it began to shine; the light ceases to be visible: if another body be applied to this when sufficiently luminous, it will also acquire from it a luminous heat.

VI. BODIES heated till they become luminous, are said to be *ignited*.

VII. SOLID bodies are rendered fluid by heat; and fluid bodies with sufficient degrees of heat are turned into vapour. But different degrees of heat are requisite to produce these effects on different bodies.

SECTION II.

The Phenomena of combustible Bodies.

HAVING premised as much as was judged necessary concerning the heat and light of uninflammable bodies, we may proceed to the subject of inflammable ones.

I. IF a sufficient heat be applied to a perfectly inflammable body *exposed to the air*, it will kindle into a flame: but this flame does not require the assistance of the cause by which it was kindled in order to the continuance of its heat and light, as is the case with ^{un}inflammable bodies. It has the property of maintaining or supporting itself till the whole of the body or substance is consumed.

II. A BODY, or vapour, when heated as above is said to be *red hot*; but the adjective should be varied: and we may with equal propriety say that the flame of sulphur is *blue hot*; the flame of copper *green hot*, and so of other colours

lours. Uninflammable bodies, in the first degree of luminous heat, emit the red-making rays most copiously, and thence are said to be red hot. If they are heated more violently, they emit all the rays in more equal proportion, and thence are said to be white hot. Thus also the flame of sulphur emits the blue-making rays most copiously; the flame of copper the green, and so of others; *shining hot*, therefore, would be a more proper general expression.

III. IN order that combustible bodies may burn, or flame, they must not only be exposed to the air, but raised into *vapour*: and even the vapour thus raised must be put into a proper state, otherwise no flame will be produced. Thus, spirit of wine may be evaporated entirely in open air, and yet no combustion happen.

IV. THE chief circumstance requisite to the inflammation of a combustible vapour in open air, is *a due degree of heat*; if that be applied to the vapour when properly compressed by the atmosphere, it inflames, in whatever manner the heat be communicated. The touch of an inflammable body already burning, or of an uninflammable

inflammable body ignited, is not necessary for that purpose.

V. DIFFERENT combustible bodies require different degrees of heat to make them burn ; for as they only burn by a flame, they must first be raised into vapour. But different inflammable substances require different degrees of heat to raise them into vapour according to their volatility : and even afterwards, this vapour is more or less difficult to be turned into flame, according as it is in its nature more or less combustible.

VI. In order that the combustion may be continued after once begun, without the assistance of extraneous heat, the body must be possessed of a sufficient quantity of the inflammable principle, or *phlogiston*; and then, if the other ingredients of that body be in due proportion, and sufficiently volatile, the combustion will continue as long as any of the substance remains; as happens with alcohol. If the phlogiston, though in sufficient quantity, be combined with matter of a fixed nature, the assistance of extraneous heat is necessary to the combustion,

as

as without it the particles of the body with which the phlogiston is combined, cannot be duly exposed to the action of the air: this happens with some metals. Vegetables, and most other combustible bodies partake of both these cases. And even after the latter operation is carried as far as possible, a substance will remain which is a truly incombustible body.—The combustion in the former case is called *inflammation*; in the latter, *calcination*.

VII. A SHINING heat in the body of the matter to be burnt has, properly speaking, no connection with its combustion. Thus iron is ignited before its combustion begins; sulphur, on the contrary, burns before it has acquired that degree of heat. Burning phosphorus cannot set fire to zinc; but zinc can inflame phosphorus long before it has acquired even a luminous heat. Different substances require different degrees of heat to begin their combustion, as mentioned before: and if the due degree of that heat be applied, provided the vapour be sufficiently inflammable, duly condensed, and exposed to the action of the air, the inflammation takes place, though the body
by

by which it is communicated be neither in actual combustion, nor ignited.

VIII. It has been sufficiently demonstrated by philosophers that combustible bodies contain a principle which *they call phlogiston*; and that this constitutes the essential difference between combustible and incombustible bodies ; I say which *they call* phlogiston, for they suppose that this principle is resolved into elementary fire by combustion, and hence they account for the *heat* and *light* attending this process : Dr. Black terms it, for this reason, *the principle of inflammability*, and others again, *the inflammable principle*. But it will appear, in the course of the following Essay, that the phlogiston is a fixed principle, of a nature very different from what it has hitherto been imagined ; that it is not fire ; and that it is only *mediately* the cause of heat in combustion. For these, and other reasons which will be seen in the sequel, I would submit to the learned whether any of the terms above mentioned ought to be continued ? and whether *electron*, or some other, ought not to be substituted in their stead ? I have, however, used the old word, till I have their approbation for adopting a new one.

SECTION III.

Of the Principle on which Combustion depends,

EXPERIMENT I.

IF alcohol be evaporated with an heat not sufficient to inflame it, and the vapour be condensed, it will be found the same substance as before.

EXP. II. IF the vapour of alcohol be inflamed, and what flies off condensed, it will not be found to be alcohol, nor even an inflammable substance; for nothing but water can be discovered in it.

COROLLARY I. BY inflammation, therefore, the vapour of alcohol is decomposed: and this holds good with all inflammable vapours.

EXP. III. IF the wick of a candle be set fire to in open air, the flame will continue until the candle is burnt.

EXP.

EXP. IV. BUT if it burns only in a certain quantity of air, the combustion will continue only during a time; which will be greater according to the quantity of air. If this air be exchanged for fresh, and the candle again lighted, it will burn only about the same time as before. By changing the air a sufficient number of times, the whole candle may be burnt out as completely as if it had not been confined in a close vessel: but no art can continue the combustion without such renewal of the air.

COROL. II. THE second experiment shewed that inflammable vapours are decomposed by combustion, and reduced to the state of *uninflammable bodies*. They were, therefore, decomposed by having their *phlogiston* taken from them. In this experiment, we find that the air takes something from the burning vapour; for after a vapour has burnt in a given quantity of air during a sufficient time, the combustion cannot any longer be continued; though if fresh air be added, it may; the air therefore was saturated with something which it had taken from the inflamed vapour; but what the
vapour

vapour lost was the principle which constituted it an inflammable substance. It was the *phlogiston* therefore which the air took from the vapour, and with which, in the end, it was saturated. Now, as the flame continued only while the air was taking the phlogiston from the vapour, and went out when the air was no longer able to do this, it seems "that the combustion depended entirely on such action of the air on the phlogiston."

EXP. V. IF a bit of charcoal be inclosed in a large vessel, and made sufficiently hot, and then the whole be suffered to cool, the air in the vessel will be found saturated with phlogiston. If fresh air be added to the coal (the first being withdrawn), the operation repeated, and so on successively for a number of times, the phlogiston of the coal will be very sensibly diminished, as I have tried. And, therefore, if the operation had been repeated a sufficient number of times, the whole of the phlogiston might have been extracted as completely as if it had been burnt in the open air.

COROL. III. A COAL is a combination of phlogiston with earth; but by this experiment

it

it appears “ that the phlogiston has a greater affinity with air than with the earth of the coal ; and therefore when the proper circumstances concur, it quits the latter to join with the former.” The circumstances which attend this are similar to what happens in other chymical decompositions. If I put a quantity of fixed alkali united with some other substance, suppose sulphur into a glass, and pour on it a little vinegar, the vinegar will extract the alkali from the sulphur until it is perfectly saturated therewith ; but even if heat be afterwards applied, it will not extract any more ; neither will air, when saturated with phlogiston, extract any more of that principle from the charcoal. If now the saturated acid be separated, and fresh poured on, more alkali will be taken from the compound ; and thus we may proceed till the whole is drawn out ; the sulphur will then remain behind, deprived of its alkali, in the same manner as the earth of the coal remained behind deprived of its phlogiston. The strength of the analogy will easily be perceived by the philosophical reader.

THE air saturated as above is called *phlogificated* and *fixable air*.

EXP. VI. IF in open air any heat be applied to *sulphur* below a certain degree, it will not burn. If sulphur be inclosed in a vessel with fixed air, and a greater degree than that with which it would be burnt in the open air be applied, it still remains uninflamed, and the same sulphur; but if the vessel with the sulphur in this state be uncovered, it kindles into a flame immediately on the admission of air; without the application of a body already burning, and is entirely decomposed.

COROL. IV. The substance which has hitherto been considered as having one of the greatest degrees of affinity with phlogiston is the vitriolic acid; for most other substances which contain that principle, part with it to this acid, when the circumstances requisite to their union concur; the substance formed by their union is *sulphur*, the subject of the above experiment. But it appears that phlogiston has a greater affinity with *air*, than with *vitriolic acid*: for, when the proper circumstances concur, it quits the latter to join with the former. The affinities of phlogiston therefore, with respect to these substances, should be placed thus:

G

Phlogiston.

Phlogiston,

Air,

Vitriolic Acid,

&c.

Now that the phlogiston is really united with the air, by means of a superior affinity, as explained above, appears not only from what has been said, but also from this consideration, that the air thus combined is altered in its specific gravity, is less elastic, and in other respects changed in its properties. The properties of the vitriolic acid are likewise altered on its combination with the same principle, with an aleali, or any other substance. The saturation of air with phlogiston, is as analogous to the saturation of the vitriolic acid with the same principle as any two processes of the kind can be, allowing for the very different natures of these substances; but the following analogy will set it in a still clearer light. Common salt, and cubic nitre, may, in this view, be considered as similar, except in the attraction which their alkaline bases have with their respective acids. The vitriolic acid decomposes the former with greater ease than the latter, because
its

its principles are united by a weaker attraction. In like manner, phosphorus and sulphur may be considered as differing from each other only in the affinity which the phlogiston has for the respective acids. But the air decomposes phosphorus with greater ease than sulphur: and for this no other reason appears but that the phlogiston has a weaker affinity with the phosphoric than with the vitriolic acid. *In the process of combustion, therefore, we must reason in the same manner as on other chymical affinities and decompositions.*

SECTION IV.

Of the Phlogiston.

THE doctrine of combustion which at present prevails is, that the phlogiston is *combined elementary fire*: that in this process it is set at liberty, and resumes its elastic state; and that the heat and light of flame, as also the property which it has of supporting or maintaining itself, proceeds from the avolation of this disengaged principle.

THE phenomena of fixed air are made use of to illustrate this theory, and from hence, indeed it was, by analogy, derived. Fixable air may be combined with various substances, and form with them gross bodies: thus it may be combined with the caustic volatile alkali; it may be transferred from thence to other substances with which it has a greater affinity, to the caustic fixed alkali for example; and from thence again to quicklime. But if a substance be applied to the compound with which the quicklime or alkali has a greater affinity than
with

with the air, it lets go the air, and unites with that substance. The air thus disengaged, and no other substance at hand with which it can unite, resumes its elastic state, and becomes permanent air; as it flies off it causes an effervescence in the liquid in which it was contained.

It is supposed that elementary fire may, in like manner, be combined with bodies, and that it may be transferred from these to others for which it has a stronger attraction: thus, it may be combined with the earth of charcoal. From thence it may be transferred to metallic * calxes; from these to the phosphoric acid, and from these again to the vitriolic. In the process of combustion, it is considered as "let go by the body with which it was combined; that it resumes its elastic or expansive state, and, by its flight, produces the phenomena of heat, &c. after the same manner as air produces effervescence." This theory is ingenious, but I think not true, for the following reason:

It is known to chymists, that pure sulphur is a combination of the vitriolic acid with phlo-

* Zinc, &c.

giston, and that these are the only ingredients which enter into the composition of that substance. In combustion the sulphur is decomposed by means of a third body, or air; which having a greater attraction for one of the ingredients than that which is already combined with it, that attracted ingredient quits the other, and unites with the air. Now if the analogy held good, we must say "that the vitriolic acid had a greater affinity with the air than with the phlogiston, and therefore let go the latter to join with the former; that the phlogiston thus disengaged, resumed its elastic state as elementary fire, and caused by its flight the phenomena of heat, &c. just as disengaged air causeth effervescence." But the reverse of this happens, for the acid is left behind; and of course "it was the other ingredient, or phlogiston, which combined with the air."

STRANGE as it may seem, this last fact is acknowledged by those very persons who embrace the theory of combustion above explained; so that it is matter of surprise that they have not discerned the perfect analogy of this process with other chymical ones of the like kind.

Perhaps

Perhaps the light which attends combustion has *dazzled their eyes*, and proved an *ignis fatuus* which has led them out of their way. Perhaps also, the property which inflammable bodies have of maintaining the combustion once begun in them, has proved their stumbling block. For we shall find that these phenomena admit of an easy solution from the doctrine above laid down.

THE *destruction* * of the phlogiston in this process has been a favourite doctrine since Stahl honoured it with his opinion. But when vinegar extracts the alkali from liver of sulphur, as in an experiment before related, chymists do not say that the alkali is destroyed, as they say that the inflammable principle is destroyed by combustion; their ideas on that head are clear enough: they rightly conclude that the alkali had left the sulphur to unite with the vinegar. They argue in the same manner on the decomposition of charcoal by the vitriolic acid! why this reasoning should have been departed from in the instance before us, is not easy to imagine.

* Vide Macquer's Chymical Dictionary.

It would be easy to bring other objections to the prevailing theory of combustion ; but as I imagine that first stated to be an *argumentum crucis* (if the expression may be allowed), it will be needless to trouble the reader with more.

PART of this theory however is true, as will hereafter appear. The *light* of flame proceeds from the disengaged phlogiston, though the *heat* does not. Also the *heat* really proceeds from disengaged fire, as chymists at present imagine ; they are only mistaken as to the origin of that fire.

SECTION V.

Of the Heat and Light attending Combustion.

IT is well known to chymists that when certain bodies unite, their combination is followed by a greater heat than what those bodies possessed before. Thus heat is generated when an acid is saturating an alkali. The like happens when water is mixed with spirit of wine; and a still greater heat follows on mixing water with the mineral acids. In some cases therefore the heat may perhaps be so great that the new compound shall be luminous. When water is mixed with quicklime, the heat is sometimes so great as to kindle combustible bodies with which it happens to be in contact. In the pyrophyrus of Homberg, the union of water with the vitriolic acid is attended by so great an heat, that the inflammable substances in the compound are set on fire by it; and the nitrous acid and oils actually inflame.

THE cause of this heat I defer the consideration of to a future section: the fact alone is sufficient for our present purpose. The heat generated by the combination of phlogiston and air may, for the present, be reckoned analogous to these; and, in ordinary combustion, is so great as to be luminous, as may be gathered from the second Corollary in the third section hereof.

IT may farther be observed that combustible bodies are heated to a degree before they begin to flame: and it may be presumed that the same quantity of heat is generated by the combination whether the particles before their union were hotter or colder. When, therefore, the ingredients are previously heated, or their temperature is higher; the heat after their combination will be greater than if they had united in a colder state, because the heat generated by their union, is added to that which they had before acquired. If this be joined with the considerations in the preceding paragraphs, it will cease to be a wonder that this process is attended with a *shining heat*.

Now

Now, in cases where the heat is not intense, as in the combustion of sulphur, I apprehend that the combined phlogiston and air only are luminous : but if it be sufficient, a shining heat will be generated in the extraneous particles of the vapour. I defer a¹ particular consideration of the light of flame to a future section, and shall here only observe that a vapour whose particles are rendered luminous, must appear to us under the form of *flame*.

S E C T I O N VI.

Of the Continuance of Combustion.

I N the foregoing pages we have endeavoured to explain the principle on which combustion depends, and the phenomena of heat and light which attend the combustion of inflammable bodies. The property which these bodies have of maintaining the combustion after it is begun, shall be the subject of this section. I shall treat this subject in as concise a manner as I did the others; and an attention to the following circumstances will sufficiently explain my ideas on that head.

LET a vapour be raised from a perfectly inflammable substance in open air, let that vapour be properly compressed by the atmosphere, and a sufficient heat applied, the particles of air will attract the phlogiston from the particles with which it was before united, and a shining heat will follow. The vapour therefore will appear under the form of *flame*. The particles

ticles thus ignited will be enabled to communicate heat to those on the surface sufficient to raise them into a vapour proper to be acted on by the air. This vapour being in like manner decomposed, these fresh ignited particles of phlogiston and air will communicate heat to those next on the surface, which therefore will likewise be elevated and decomposed; and so on in a continual succession, as long as any of the substance remains: for, as by the combination of each particle of air with phlogiston, heat is generated, and that in the great degree mentioned above, so many particles as thus combine, so many new sources or springs of heat will there be; which, with what follows, will be amply sufficient to account for the phenomenon in question.

FOR this process is assisted or facilitated by the action of the atmosphere, by which the vapour is compressed, and the particles of air and phlogiston forced into contact: hence the combustion goes on in an heavy atmosphere better than in a light one: Hence also, when bodies burn in close vessels, the flame ceases before the air is all saturated with phlogiston, because its elasticity

ticity being weakened, the vapour is not sufficiently compressed. Bellows, and currents of air, besides that they drive away the saturated air, and apply fresh particles to the vapour, assist combustion on this principle.

It appears, from what has been already said, that different combustible bodies require different degrees of heat to make them flame. The degree of heat therefore which is necessary to begin the combustion will, for the same reason, be required for its continuance; now, whatever this requisite degree of heat be, yet if the body be perfectly and uniformly inflammable, or burns wholly away when once kindled, it will be found that *more phlogiston and air are combined in a given time*; and therefore a greater quantity of heat continually generated, sufficient to equal that first degree of heat, and of course to maintain the combustion. We have, therefore, from the above principles, a very easy solution of the phenomena of combustion, and the theory will perhaps be the more readily embraced, as its principles are within the bounds of common observation.

THE reader will easily apply the doctrine delivered to the particular phenomena of combustion; with an instance of which I shall conclude this section.

EXPERIMENT. If, instead of air, *nitre* be mixed with a combustible body, and put into a close vessel in vacuo, or otherwise, and then made sufficiently hot, the combustion of that body will be as complete, as if it had been burnt by means of air. •

THE nitrous acid, therefore, contains a quantity of air of the same nature with that of the atmosphere, but in a combined state: as soon as the proper circumstances concur, the phlogiston in the inflammable body, and the air in the nitrous acid, by a mutual attraction are withdrawn from the substances with which they were before combined, and unite with a shining heat, in the same manner as atmospheric air and phlogiston in the instances described. The air thus at liberty, resumes its elastic state, and becomes the same fixed air as that generated by common combustion. The phenomena of gun-powder and other nitrous combustions

combustions may be understood by means of this theory.

CHEMISTS have observed, that if phlogiston be combined with the phosphoric or vitriolic acids, *fulphurs* are formed: as nitrous acid has a greater affinity with phlogiston than either of these, they conclude that a sulphur is likewise formed by their combination. "But (say they) the nitrous acid and phlogiston unite with such violence, that the sulphur is destroyed the very instant that it is formed *." Is not the fixable air produced in this process the nitrous sulphur? and would it not be consistent with chymical analogy, and therefore more proper, to call fixable air in general Aërial Sulphur? but this is spoken with submission to better judges.

It has generally been supposed that the great attraction which the nitrous acid has for phlogiston, is owing to its containing that principle as a *constituent part* †. But the reason now appears to be that it contains common *atmosphé-*

* Macquer's Chymical Dictionary. † Ib.

rical air; I take it that it contains phlogiston in no other manner than as the volatile vitriolic acid does; and that by exposing to the air the phlogiston is dissipated; for it ceases to fume, and becomes fixed like the oil of vitriol. Perhaps air constitutes the *essential part* of the nitrous acid, on which its taste, corrosiveness, and other general properties depend; and it seems to me that it is combined with nothing but water by means of an earth: for water only is to be found in the nitrous clyffus, and the earth may be left behind with the alkali. I used to think that it was combined with water alone: but if the reasoning in a following section on air be admitted, that cannot be the case; for water parts with air with a less degree of heat than nitre does, and therefore there must be some other substance of a more fixed nature with which it is combined, and only mediately with the water by means of that substance. Is the essential part of the *vitriolic acid* also atmospherical air in a state of combination *, but combined with such sub-

* Do not the explosions which have been observed of balsam of sulphur favour this opinion? did the air and phlogiston mutually disengage each other, and form fixed air?

stances, or in such a manner, that it is not separable by the phlogiston, as in the nitrous? and is this the reason of its great affinity with phlogiston? and may the like query be put concerning the other acids? but this by way of digression.

FROM what has been said, it appears that the phenomena of combustion depends on this principle; that air has a greater affinity with phlogiston than the substances have with which it is combined in inflammable bodies, and therefore when all circumstances properly concur, it attracts that principle from those bodies; that a shining heat is generated by their combination; and that this decomposition when once sufficiently begun in a perfectly inflammable body, together with the shining heat which is a consequence thereof, will be continued on the principles above laid down, without any farther assistance from extraneous heat, as long as any of the substance remains.

WHEN phlogiston is combined with the earth of charcoal, with the vitriolic acid, or certain other substances, a combustion may happen by
reason

reason that the air can attract it from these substances; but when it is combined with air no combustion can happen, because air cannot attract phlogiston from other air, any more than the vitriolic acid can attract it from sulphur; for the affinities are equal, and one substance cannot attract another from a third, with which it is combined, but by means of a superior affinity. This may also be applied to the nitrous sulphur above spoken of.

THERE are however, certainly, substances capable of attracting the phlogiston from air, otherwise the whole atmosphere would in time be converted into fixable air. The ingenious Dr. Priestley, to whose labours the learned world is so greatly indebted, has already discovered two of these means: he shews that fixed air may be rendered pure by vegetables growing in it, and by water. It may perhaps be added, that as fixable air has a greater specific gravity than common air, and therefore naturally tends downwards, it enters into the earth, and combines with such bodies as may be disposed to receive it. That some substances may have the property of depriving it of its phlogiston,

which then enters into their composition ; and perhaps, in some cases, there may be no other method of combining that principle with bodies, at least in certain manners. The phlogiston, therefore, when combined with air, seems to be in the most proper state for certain intimate combinations of it with vegetable and other substances; as it is probably reduced to its integral parts. Hence we have some idea not only of the manner in which fixable air is deprived of its phlogiston, but also of the circulation of the last mentioned principle, from bodies to air, and from air again into bodies. Perhaps also the fixed air, when deprived of its phlogiston, may, in some cases, be converted into acids, if the above queries concerning acids be true; but these things remain to be inquired into.

SECTION VII.

A Speculation.*

AIR has usually been reckoned a fluid *sui generis*, and called, in contradistinction to others which are coherent, an *elastic fluid*.

I HAVE long been of opinion that the elasticity of air depends on heat; for if the heat be increased, the elasticity is increased; if it be

* In the Essay on the senses I allotted a section for such hints and conjectures as had occurred to me on those subjects, in order to their being further inquired into by others: I shall devote this section to a like purpose. As I do not pretend to offer these conjectures as demonstrated truths, any errors will be pardoned by the candid, as the detection of them by experiments may lead to real discoveries; I have, for my amusement, carried the ideas contained in this section, as well as others which are not mentioned, to greater lengths. If this short extract be approved, I may in a future edition render it more copious. Those, however, who do not relish speculative reasoning may pass over this section.

diminished, the elasticity is also diminished in a certain regular proportion: it should seem, therefore, that if air was entirely deprived of heat, its particles would become coherent.

WHEN water is raised into vapour it is also elastic, and its elasticity is greater as more heat is afterwards applied. The vapour of water, therefore, is of a similar nature to air: the only difference, in this respect, between them is, that water requires a vast deal more heat to render it elastic. The like may be observed of other bodies *.

IF we imagine two particles in contact, and that heat be applied, the heat will force them to quit each other. The particles, while within the spheres of their cohering forces, will resist the action of heat more, as their cohering forces are stronger. But when the heat becomes so great as to force them beyond those spheres, they will be elastic, like air; their elasticity will be greater as the heat is increased, and that in a certain regular proportion; as mentioned above.

* It seems to me that this proposition may be made as general as Mr. Braun's concerning the fluidity of bodies.

CASE

CASE I. Heat, therefore, is the cause of the repulsive affection among particles of air; and if this cause be removed, the particles have no such tendency.

CASE II. Fire surrounds a particle of air in the manner of an atmosphere; it is denser near the particle, and rarer at a distance from it; and hence the repulsive power of particles of air. Fire therefore is attracted by the particles of air.

CASE III. When air is hotter it is more elastic; that is, its particles are surrounded with greater and denser atmospheres of fire, and therefore their repulsive powers are stronger.

CASE IV. As our atmosphere, by its gravity, is denser, or more compressed near the earth, than at a distance from it, and that in a direct proportion; so by the gravity of fire towards a particle of air, the fire is in like manner compressed. The density diminishes according to the distance from the particle; and hence the repulsive force of particles of air is directly as the distance of these particles from each other.

CASE V. When the quantity of fire composing the atmosphere of a particle is greater, the compression or density of the fire near the particle will be greater, just as happens with our air : and therefore the repulsive force of the particle will be increased with the heat, and will also reach to a greater distance *.

CASE VI. If two similar particles of air, but with unequal atmospheres, be brought sufficiently near to each other, the particle which has the greatest atmosphere will part with fire to that which has least, till their quantities are equal. The like may be observed of other homogeneous particles.

THE reason of this is obvious; the attraction for fire, or the gravitation of fire towards them, being equal in both or all the particles.

CASE VII. IF by any means the gravity of fire towards a particle, or, if you will, the at-

* It is obvious from hence, that the repulsion at the same distance from the particle does not increase in a direct proportion with the heat, but in a ratio which will easily occur to the mathematician.

traction of a particle for fire be weakened, the atmosphere which that particle retains at the same common temperature will be less in proportion thereto, and it will also be less dense; so that its repulsive power will likewise be diminished.

THE idea on which this case is founded was suggested to me by the following consideration.

IT has long been known that lead by calcination acquires considerable weight; M. Margraaf has discovered that the acid left behind after the combustion of phosphorus is almost half as heavy again as the phosphorus employed: and yet in both these cases, many particles must have escaped besides the phlogiston. I am told that Dr. Black has made experiments on metals with acids which agree so well with these that he is convinced of the truth of the inference which has been drawn from them, viz. "That the gravity of bodies is diminished by their combination with phlogiston."

THE cause of gravity, as conjectured by Sir Isaac Newton, and now generally supposed by philosophers,

philosophers, is a very subtle elastic medium, which is rarer near a particle of matter, and denser at a distance from it. That therefore two such particles will be mutually impelled by the denser, towards the rarer parts of this medium, and in course towards each other.

As the æther is *rarer near*, and *denser at a distance from*, a particle, it shews that there is a *mutual repulsion* between the particles of bodies and this fluid.

THAT, therefore, according as this *mutual repulsion* is greater, the rarity of the medium near the particles, and the force of the particles' gravity, will be greater; and as the *mutual repulsion* is less, the æther near the particles will be less rare, and their gravity diminished.

IT therefore appeared to me, that as phlogiston when combined with the particles of bodies diminishes their gravity, it does it *by weakening the repulsion between these particles and æther*.

TILL now, I had imagined, with Dr. Black *,

* To some notes which a friend had taken while attending the lectures of that great physician and philosopher (I wish they

and others, that fire, æther, and phlogiston were one and the same fluid: but on applying this reasoning to my notion concerning the repulsive force of the particles of air, I found that it entirely clashed with it; for phlogiston weakens the elasticity of air. And now, for the first time, it occurred to me that the disposition of particles of bodies towards æther and fire are quite opposite. For whereas fire gravitates towards, or is attracted by those particles, æther on the contrary is repelled; and this also led me to consider that æther causes the gravity or attraction of particles towards each other; fire, on the contrary, their repulsion. It followed, therefore, that if phlogiston diminished the repulsion between the particles of bodies and æther, *and thereby lessened the mutual gravity of these particles*; it must on the contrary *diminish the attraction between these particles and fire, and of course weaken their mutual repulsion.*

they had been more perfect), and to extracts from Dr. Priestley's discoveries concerning air, I owe my having been enabled to work this latter part of my Essay into its present form.

THIS suggested to me another idea. If particles, when combined with phlogiston, have their attraction for fire diminished, it should follow, that the same quantity of fire added to a phlogisticated, and an unphlogisticated particle, would heat the former most; because it would be less forcibly retained by that particle than by the other, and therefore a greater quantity would be communicated to a third and colder particle applied. To satisfy myself with regard to the justness of this idea, I made the following experiments.

EXPERIMENT I. In an iron pot filled with sand, heated over the fire, I placed, at a small distance from each other, two gallipots, one of which contained water, the other lamp-oil, so that they were in equal degrees of heat; after they had remained some time, I found that the oil had acquired a greater heat than the water.

Exp. II. I made the same experiment with minium and lead, and found that the metal acquired a greater heat than the calx.

Exp.

Exp. III. I tried the same with several other substances, whose specific gravities would admit of the experiment, with the like result.

I ENDEAVOURED to make the experiment with common and fixable air; but the fluids being so rare, and not properly confined, and the heat of the containing vessels so much interfering, I could do nothing to my satisfaction; and therefore could only infer from analogy; for the only thermometers that I used were conic scurvy-grass phials, and Daffy's elixir bottles, with spirit of wine in them, the surfaces of which I marked on the outside with ink. The three first experiments however, and the consideration that fixed air is less elastic than common air, seemed sufficiently to establish the truth of the proposition which they were intended to determine.

So then instead of æther, fire, and phlogiston being the same, as is at present supposed, they appear to be three distinct and very different fluids; and their relations or affections towards the particles of bodies and each other, seem to be as follows.

I. SINCE when the particles of bodies contain least phlogiston, fire gravitates towards them most, there is a mutual attraction between the particles of bodies and fire.

II. AND since when the particles of bodies are most free from phlogiston, æther avoids them most, there is a mutual repulsion between those particles and æther.

III. PHLOGISTON and æther therefore mutually attract each other ; and on the contrary.

IV. THERE is a mutual repulsion between phlogiston and fire.

PHLOGISTON therefore is to æther, what the particles of bodies are to fire : for as fire proceeds from dense to rare in its progress from the latter, so æther proceeds from dense to rare in its progress from the particles of the former. It seems then that there are two different, and as it were opposite kinds of substance of which bodies are composed, and two elastic fluids answerable to them. And if this be true, we must reckon four general principles, viz. *æther*, *fire*, *phlogiston*, and *the particles of bodies* ; but

as this last name is too general, suppose we call what is meant by them *earth*. Perhaps, however, æther and phlogiston may be composed of the same matter, and so may fire, and earth: perhaps all four may be only different modifications of the same substance; for we must go beyond fire and æther before we arrive at the ultimate principles of nature. There may be still subtiler principles on which the elasticities, and other properties of these depend.

THESE four substances may perhaps be considered as the *four elements* with more propriety than those of Aristotle which so long prevailed, and perhaps the phenomena of nature may be better understood by means of them than they are at present.

THE proportion of earth in nature seems to be much greater than that of phlogiston, and the proportion of æther much greater than that of fire: the gravity of bodies towards the earth, I think, proves this.

ALSO phlogiston appears to be much more subtile than earth, and æther than fire; for the
elastic

elastic fluid formed by particles of earth with atmospheres of fire, is much grosser than that formed by particles of phlogiston with atmospheres of æther. Thus, light is more subtile than air, and electricity than fixable air; the analogy between these may hercafter be shewn.

PARTICLES of phlogiston freest from earth attract and retain the greatest atmospheres of æther; and particles of earth freest from phlogiston attract and retain the greatest atmospheres of fire. Hence particles of earth, when most free from phlogiston, are most elastic or repulsive; and so are particles of phlogiston when freest from earth.

THOUGH earth attracts fire, which is repelled by phlogiston, yet there is a stronger attraction between phlogiston and earth, than between any other two of the principles.

ONE particle of earth cannot cohere with another, unless one or both be previously combined with a sufficient quantity of phlogiston. The phlogiston both attracts the particles of earth, and disperses their atmospheres of fire,
which

which kept them afunder *. Earth, for a like reason, is the principle of cohesion between the particles of phlogiston.

SUPPOSE a particle of earth, and another of phlogiston, with each its proper atmosphere of fire and æther, if they could be forced into combination, they would quit their attractions for æther and fire, and exert their forces on each other; or they would lose so much of their attractions for those mediums (and therefore of the atmospheres) as they exerted on each.

* If phlogiston be supposed to compose the cohering forces of the particles of earth, the ætherial atmospheres of the particles of this phlogiston, though greatly decreased by being combined with earth, will yet extend to a little distance beyond the cohering atmosphere sufficiently strong for producing a sensible effect, and will furnish us perhaps with the cause of the repulsive force observed by Sir Isaac Newton, viz. that two object glasses will lie on one another without touching; that two polished marbles are with difficulty made to cohere; that beyond the cohering forces of bodies there is a repulsion; that the rays of light are inflected, reflected, and refracted by bodies, and the like. The less forcibly the phlogiston is combined, the greater must these effects be.

BUT pure earth and phlogiston cannot directly combine, by reason that their atmospheres hinder their union. Thus light cannot be combined directly with air; but if the light be presented to a particle of earth already combined with a proper quantity of phlogiston, whereby its atmosphere of fire may be sufficiently diminished, the light can enter into combination with it, and then air, by reason of a superior attraction, can take it from that particle. The like may be observed of phlogiston previously combined with earth: The solution of some curious phenomena seem to depend on this principle, as may hereafter be shewn, if this specimen be approved.

IT may be proper to observe that there is not a perfect analogy between earth and æther, and phlogiston and fire, as may at first view be imagined. The quantity of fire in the universe seems to be very small, and to be only confined to the planets and other heavenly bodies; round the earthy particles of which it forms atmospheres, as hath been described, and perhaps of no very great extent even in particles of air. But æther is in quantity vastly superior; and
as

as its great supposer says, "is expanded throughout all the heavens;" particles of earth therefore will have, besides their limited, decreasing, or repulsive atmospheres of fire, increasing or attractive atmospheres of æther, extending perhaps to the utmost bounds of the universe. But particles of phlogiston will have decreasing or repulsive atmospheres of æther, reaching to the same distance as these last, but no increasing or attractive atmospheres of fire, or at least only momentary ones; because, on account of the small quantity of this medium, and its not filling the universe, it will all be attracted by, and gathered about the particles of earth. To illustrate this it may be noted, that when to a particle of earth another of phlogiston is added, part of its repulsive atmosphere of fire is dislodged. Now, if the analogy held good, the dislodged fire ought to go into the increasing or attractive atmosphere of fire of the particle of phlogiston added; but, on the contrary, it goes into the repulsive atmospheres of fire of the particles of earth around, as is proved by the thermometer and the sense *. On the contrary, when to a particle of phlogiston another of

* Vide case XI.

earth is added, the æther which is expelled from the repulsive atmosphere of the former, goes into the attracting atmosphere of the latter. For when oil of vitriol is mixed with water, fire is dislodged from the particles of earth; and by the same reason æther must be dislodged from the particles of phlogiston. Now heat is caused by the dislodged fire, because it goes into the repelling atmospheres of fire of the surrounding particles of earth, as observed above *. But if the same rule held good with the dislodged æther, the mixture would weigh heavier than the ingredients did before, because the particles of phlogiston having less repulsive atmospheres of æther, would be less repelled by the globe of the earth, and therefore they would have less levitation, or centrifugal force. But the æther dislodged from the repulsive atmospheres of the particles of phlogiston, goes into the attracting atmospheres of the particles of earth with which the phlogiston is combined, and which therefore by that combination had

* Perhaps Phlogiston does not repel fire atmospherically, but by particle and particle, their subtilty being nearly alike. Or do they really *repel*, or only *expel* one another, as the fixed aleali expels the volatile from acids?

its repulsion for æther diminished; and therefore what the particles of phlogiston lost in centrifugal, those of earth lost in centripetal force, so that the weight continued the same *. When these ideas first occurred to me, I made experiments with oil of vitriol and water, and with spirit of nitre and ice, to see whether they altered in weight after mixture. By the inaccuracy of my weights I had like to have fallen into an error, for the vitriolic mixture seemed heavier, and the nitrous lighter than their ingredients; but by repeating the experiment I discovered the cause to be in the weights. The absolute gravities of the compounds were the same as those of their ingredients; and consequently the decrease or increase of attraction of æther by the particles of phlogiston, was balanced by an

* Imagine a particle of phlogiston where gravity is -1 , and another of earth whose gravity is -2 . If they are combined, their gravity will be equal to the sum of their gravities before combination, or -3 : and this will be the case whether their combination be more or less intimate; and whether free particles with their full atmospheres be supposed, or particles already combined; for on their separation from their previous combinations, they will instantly acquire their proper ætherial atmospheres, as is obvious from what has been said.

equal increase or decrease of the repulsion of that medium by the particles of earth.

THE principles which have been proposed are, probably, of general extent. All bodies may be compounds of phlogiston and earth, with regulating atmospheres of æther and fire ; and all the differences in these bodies may arise from the different proportions and manners of their combination. A new field of speculation seems therefore to be opened to philosophers by this theory.

CASE VIII. But if the mutual attraction be increased, the atmosphere of the particle, and also its repulsive power will be augmented.

CASE IX. Supposing two particles of air A and B, and that the gravity of fire towards B is decreased, so as to be but half of that towards A ; the atmosphere of A will contain twice the quantity of fire of the atmosphere of B. If these two particles be brought near to each other, the atmospheres will not become equal, as in case VI. but each particle will retain its atmosphere as before.

THE

THE reason of this likewise is obvious; the attraction of A for fire being double that of B, and their repulsive powers will be different.

CASE X. The same things being supposed, the heat of B will be equal to that of A, notwithstanding it contains but half the quantity of fire.

FOR these are the proportions which they would retain at the common temperature, or when placed near each other, as above : and this rule determines the heat, as is evident by what follows.

CASE XI. The same being supposed, an equal quantity of fire added to A and B, will heat B twice as much as A ; and the quantity of fire necessary to raise them to equal heats, will be in proportion to the quantities of fire which, at the common temperature, they naturally retain. The like may be observed with regard to cold.

FOR if to A a third particle were applied, which had but half the heat, but which would

naturally retain as much fire as A, it would take one fourth of A's fire from it, by case VI.

BUT if the same particle were applied to B, it would take away half of B's fire to raise it to the above heat, though one third only would render their quantities as 2 to 1; as is obvious by what was said in the IX. and X. cases.

THE mixture of bodies, which at the common temperature retain different proportions of fire, when these bodies are heated at different degrees, and the phenomena resulting from them, as also the equal affection of the sense, and of the thermometer by those different bodies when at like temperatures, may be understood from this case, and those which precede it.

DEFINITION I. When, at the common temperature, a particle is made to retain a greater quantity of fire than it would naturally do in that temperature, the particle shall be hot.

DEF.

DEF. II. And if it be made to retain less fire than it naturally would do in that temperature, it shall be cold.

BODIES are expanded by heat merely because their particles are surrounded with greater atmospheres of fire, and therefore repel each other, so that they are kept at a greater distance than before. Cold is caused in the sense merely by diminishing, and heat merely by increasing the quantity of fire in the part, and therefore causing a like contraction or expansion of that part *. I do not therefore see any reason for supposing either that the particles of bodies are in a state of vibration when hot, or that the particles of fire themselves are in that continual rapid motion which others imagine †. If the latter was the case, the particles of air ought to be exceedingly hot, by reason of the great and therefore condensed atmospheres of fire which they contain ; but at the common

* It must be observed that pain, which accompanies these sensations when violent, is not to be confounded with the sensations themselves.

† Vide Macquer's Chymical Dictionary.

temperature

temperature they are no hotter than others which retain atmospheres much less.

CASE XII. Imagine a particle attracting fire, and another which would diminish that attraction; if they are at a sufficient distance from each other, the latter will not affect the attraction of the former for fire, but it will diminish that attraction more on being brought nearer; and when they meet, the diminution will be greatest of all.

CASE XIII. The same things supposed, the attraction of the former particle for fire will be less, according as it is already combined with more of the latter.

CASE XIV. The attraction of the former particle for those of the latter kind will be less, according as it is already more saturated with them; for they will exist in the atmosphere at a greater distance from the particle; and therefore they will also have less power of diminishing the particles attraction for fire *.

* Vide Case XII.

THESE cases were suggested to me by the following experiments.

EXP. I. It is well known that if oil of vitriol and water be combined, a great degree of heat is generated.

IN a small slender phial I put water, and added to it about an equal bulk of oil of vitriol; the acid was poured down the sides of the phial, and remained at the bottom; as soon as this was done, and before shaking them together, I marked the height of the liquid on the outside, and then well mixed the ingredients. A very great heat presently succeeded, and afterwards I found that the surface of the liquid was below the mark. But it could not have evaporated, because it was close stopped with a cork *.

EXP. II. I put some pounded ice into a phial,

* I have somewhere read that a drop of concentrated vitriolic acid, and another of water being put into a slender tube, penetrated each others dimensions, so as to be less in bulk. But the specific gravity of oil of vitriol first led me to try the above experiment.

and

and added spirit of nitre highly concentrated till it just covered the ice; I immediately marked the height of the liquid; and after the ice was dissolved, found, contrary to what happened with the above mixture, that the liquid had risen above the mark. A great degree of cold was generated by the combination.

I do not remember that there was any inaccuracy in making these experiments; and they seem to indicate that the heat and cold in these mixtures are connected with the contraction and expansion of the compound. I at first thought that it was from the contraction or expansion of the body as a whole; but on considering that ice is more expanded than the water from which it was formed, and yet that heat is generated by the congelation *, I concluded that the contraction and expansion must be considered as in the particles themselves; and that this always takes place in the particles on these occasions, though particular circumstances (such as new arrangements of the particles, and the like) may in some cases hinder the rule from obtaining in the whole mass or body.

* Dr. Black.

THE particles of the oil of vitriol and water therefore were, by some very powerful agent, drawn nearer to each other, so as to occupy less space than before; and as the principle of cohesion in earth, or common gravitating matter, was shewn to be phlogiston, it seems to have been effected by the agency of that principle. As the phial was corked, it did not seem likely that any fresh particles of phlogiston should have been derived from without. Besides, if that had been the case, the weight of the compound would have been lessened: but by repeating the experiment, and weighing the ingredients before mixture, and again immediately after, and suffering the whole to remain in the scale, properly suspended, till cold, I did not find this to happen. Now phlogiston must combine more firmly with the bodies according as they are already less saturated therewith * That water contains this principle in considerable

* Imagine a particle of earth, and that phlogiston be added to it in the manner of an atmosphere. The particles of phlogiston at the greatest distance, being less attracted, will retain greater atmospheres of æther. Phlogiston may be added to the particle till it can retain no more, by reason of the repulsion

considerable quantity is obvious, by its affording nourishment to vegetables, by its being a conductor of electricity, and also by an experiment of Dr. Priestley, in which the calx of mercury was reduced by the phlogiston from that fluid, and this also shews that it was not contained in a strongly combined state. The oil of vitriol, by reason of the vitriolic acid *, is of a more pure earthy nature, or is less phlogistigated; and therefore its attraction for phlogiston will be greater than that of water. The phlogiston of the water will therefore be laid hold of by the acid, and that still retaining the water, a close and intimate connection, or strong attraction, or cohesion will take place between the particles of the acid and those of the water, so that they will be drawn into lesser dimensions. By this more intimate combination the

pulsion of the ætherial atmospheres. For the same reason two such particles, when overcharged with phlogiston, will have their cohesion diminished by fresh addition instead of increased. A particle thus overcharged, and another charged less, will cohere more strongly than the two particles just mentioned, for the outer phlogiston of the former particle will be more forcibly attracted by the latter, than by any homogeneous one, and therefore will more firmly combine with it.

* Vide section VI.

particles of phlogiston will lose part of their attraction for æther ; and, for the same reason, the earthy particles of the acid applied, will lose part of their attraction for fire. *The fire which thus becomes superabundant is, I take it, the cause of the heat of the mixture.*

IF we imagine the oil of vitriol to be again separated from the water, a degree of cold will be generated, equal to the heat from their mixture, because the attraction of the earthy particles for fire will be restored.

THIS, and what follows to case XVII. will probably explain the causes of heat and cold arising from chymical mixtures in general.

CASE XV. It seems therefore that “ whenever heat is generated, without any addition of fresh phlogiston, it argues an increase of attraction between the particles of phlogiston and earth, and a consequent diminution of the attraction between the particles of earth and fire.” The contrary may be observed with respect to cold.

WHEN Bodies return from an elastic to a fluid state, or from a fluid to a solid state, heat is generated ; and cold in the contrary cases, as Dr. Black and others have shewn ; the above may be the reason. And there are other phenomena of the kind which will occur to the Reader, probably depending on the same principle.

CASE XVI. When a particle of phlogiston is combined with a particle of earth, heat is generated, if the foregoing reasoning be true ; and if the combination be rendered still more intimate, fresh heat will arise.

CASE XVII. It follows therefore, that if phlogiston weakly combined with one particle, be transferred from thence to another, with which it may form a stronger, or more intimate combination ; the heat generated by the combination, in the latter case, will be greater than the cold generated by the decomposition in the former : and this difference will be greater, according to the difference of the two attractions or combinations. The contrary may be observed of the generation of cold ; and the

the reasons are obvious from what has been said.

THIS case may obtain in some kinds of chymical mixtures, particularly in certain solutions of metals by acids. &c. But the former part of it seems to me to be the principle on which the heat in combustion depends.

THAT phlogiston has different degrees of attraction or forces of combination, with different particles, and with the same particle * in different circumstances, appears by the fol-

* Vide case XII. &c. It may be observed that the attractive forces of spherical bodies decrease in the duplicate ratio of the distance. The rarity of the phlogiston therefore must increase in the direct proportion of the distance, as happens with our atmosphere, and with the atmospheres of fire about particles of earth, as shewn before. Sir Isaac Newton has shewn, in the 369 page of his Optics, 3d edition, that the attraction of cohesion is as the distance; which answers to the above. Particles of earth do not seem to exert their whole attractive force on phlogiston; but after saturation with it they seem to have some attraction left for fire, as water saturated with one salt can yet attract another: hence the cohering forces of bodies reach but to a given distance beyond them. The like may perhaps be the case with phlogiston.

lowing considerations. After phosphorus is burnt, if the acid be urged with a great heat, it will give evident signs of containing phlogiston, as Mr. Macquer observes. Lead easily parts with a certain portion of its phlogiston by calcination; but retains another part very obstinately. Phlogiston with pure vitriolic acid, forms sulphur; but if water be previously combined with the acid, it forms an incombustible liquid. Light cannot form a combination directly with air; but if it be previously combined with proper matters, it is then combustible phlogiston. Phlogiston transferred from charcoal to zinc is much less easily combustible than while it was in the charcoal, and many other instances might be produced.

PARTICLES of greater fixity or force of cohesion (I mean homogeneous ones, and in certain circumstances) appear to have less atmospheres of fire than those homogeneous ones whose fixity, or force of cohesion, among one another is less. Hence we find that the particles of water, for example, have atmospheres of fire so small that with the usual heat of the air, their spheres of repulsion do not reach beyond

yond their spheres of cohesion *. For in that heat they will coalesce after having been rendered elastic by a greater degree; and the particles of many bodies coalesce in an heat much greater. On the contrary, air, which is a vast deal less fixed, or the cohesion of whose particles one with another is a vast deal less, is very elastic with the usual heat of the atmosphere, and even in the greatest cold we have been able to produce. The atmospheres of fire therefore extend to a greater distance beyond the cohering spheres of the particles, and a prodigious quantity of fire seems to be contained in the atmosphere of a particle of air, in proportion to what is retained by one of the fixed particles just mentioned, insomuch that air seems to be the great reservoir of this principle. If, therefore, we suppose the cohering spheres of particles to be composed of phlogiston, and as phlogiston seems, by what has been said, to weaken the attraction of particles for fire, we have a reason why fixed or cohering particles have less atmospheres of fire than those which are less so: homogeneous particles, however, must be understood in these cases; for

* See also section IX.

with heterogeneous ones the case will be different *. Also, even homogeneous particles overcharged with phlogiston will not cohere so well as when they have less, as appears from what has been said; and yet their attraction for fire will be less.

IT seems to me that the fixed or unalterable particles of bodies, as of air, water, &c. † are not pure gravitating matter, or earth, but particles of earth and phlogiston combined together, in different proportions and manners, so as that they may have, originally, greater or less attractions for phlogiston and fire, and repulsion for æther. What was advanced in case XII. &c. will hold good with any of these, but with different degrees of force, and the idea, properly pursued, might have its uses. But however this be, the fact, that *moveable phlogis-*

* Thus air will cohere strongly with more fixed particles (as in nitre), though its particles will not cohere among themselves: the reason is plain from what was said in case XIV.

† I do not mean the primary or absolutely solid particles, but these on which the invariable secondary properties of bodies depend, and which experience shews to be indestructible. Thus water is the same in all ages.

ton * is more intimately combined in some bodies than in others, and that it weakens the attraction for fire in proportion to the force of that combination, seems probable from what has been said, and will appear still more probable when we apply it to the solution of the phenomena of combustion in the next section. As I do not pretend to demonstration, and as these cases contain as much as is judged necessary to the subject which they were intended to elucidate, I shall not here pursue the idea any farther.

WHAT has been said of the attraction of fire by particles of earth in the foregoing cases, may be applied, under proper restrictions, to the repulsion of æther by the same particles, and to the attraction of it by particles of phlogiston, as is evident from the preceding discourse, and therefore I need not enlarge on it.

* I mean phlogiston which may be transferred from one body to another.

SECTION VIII.

Of the Origin of Heat in Combustion.

HAVING in the preceding section given some conjectures concerning the manner in which fire is retained by the particles of bodies, and by what laws it is regulated; we may proceed to examine how far they agree with the heat which attends combustion.

IT was shewn in the third section that combustion is a truly chymical process, and depends on this principle, “that the air attracts phlogiston from the combustible body by means of a superior affinity.”

IN the fourth section it appeared that when a particle of air attracted phlogiston from a particle of a combustible body, heat was generated, and it was promised that the cause of that heat should be considered in a future section.

IT

IT has appeared probable, in the course of the last section, that the particles of bodies have an attraction for fire; and that the attraction is greater, as the particles are freer from phlogiston. It further appeared, that according as the same quantity of phlogiston is more intimately combined, it causeth a greater diminution of the particles' attraction for fire. It also appeared that the particles of air have very great attractions for fire, and thereby retain vast atmospheres of it; but that the quantity of fire retained by fixed homogeneous particles is on the contrary very little,

Now, agreeable to the seventeenth case in the preceding section, imagine a particle of the phosphoric acid saturated with phlogiston, and that it be applied to a particle of air: let the attraction of the acid for phlogiston be weak, but that of air strong, the particle of the air will attract the phlogiston from the acid; and the combination will be stronger than with the acid in proportion to the difference of the attractions; the heat generated by the combination, will be greater than the cold generated by the decomposition, for the same reasons: in

other words, the superabundant fire of the air will be more than sufficient to satiate the increased attraction of the acid, by the excess of the attraction of the air above that of the acid. And this seems to me to be the manner in which heat in combustion is generated.

COROLLARY I. The difference of the attractions may be estimated, by measuring the generated heat, and attending to the following circumstance.

COR. II. The superabundant fire of the air will be attracted by, or gravitate towards, the bodies around, in order to restore the equilibrium. And according as these surrounding bodies, or the extraneous particles, have less attractions for fire, the more will they be heated by this superabundant fire. Vide case XI.

COR. III. Supposing a number or mass of the phosphoric particles, the more of them that are decomposed in a given time, the greater will be the quantity of superabundant fire generated in that time; and therefore, supposing the surrounding bodies, or extraneous particles to be
the

the same, the hotter will they be: but if they be different, their heat will be different according to their attractions for fire, as may be collected from the eleventh case in the last section.

Now that fire is taken from the air in combustion appears from hence, that its elasticity, or repulsive power is weakened, and its dimensions decreased, so that the particles are not kept at so great a distance from each other as before; for it was shewn in the first five cases that the particles are kept at a distance from each other by means of their atmospheres of fire. Mr. Cavendish has shewn, if I remember right, that the specific gravity of fixable air is to that of common air as $1\frac{1}{2}$ to 1, and yet the absolute gravity of the fixed air must have been greatly diminished by its combination with phlogiston, so that the difference of elasticity of pure fixable and common air must be still greater than in that proportion*. Those who have conveniencies for making experiments would do well to examine and ascertain these matters.

* See also the next section.

IF we take a survey of the different inflammable bodies we shall find that some of them require a greater heat to kindle them, and others a less. And therefore before a body can be enabled to continue its own combustion, such a quantity of its particles must be decomposed in a given time, as will be sufficient to furnish a due portion of superabundant fire. But, if the substance be perfectly and uniformly inflammable, a sufficient number of particles thus once decomposed, will furnish fire enough to equal the degree of extraneous heat first applied, and therefore to continue the generation of a like quantity successively, by means of new decompositions, as long as any of the substance remains. Vide section VI.

SUPPOSING an equal quantity of superabundant fire generated by different bodies in the same time, "the heat of the flame will be greatest in those whose phlogisticated particles, after parting with their phlogiston, have the least attraction for fire;—which contain the fewest extraneous particles—and whose extraneous particles attract fire least." —
 —The flame of spirit of wine, when sufficiently gentle, is not even ignited, as will be
 shown

shewn. For this liquid contains so large a proportion of extraneous particles *, and these seem to have so great an attraction for fire, that the superabundant fire is not sufficient to make them red hot. Perhaps there is not more fire separated from the air in a given time by oil, than by spirit of wine: but as oil contains a much less proportion of extraneous particles, the same quantity of fire is sufficient to ignite the vapour of oil, though it cannot that of spirit of wine.

IF we suppose all other circumstances alike, the heat of the flame will be greatest in those bodies which, in a given time, saturate the greatest quantity of air, or generate the greatest quantity of superabundant fire. Perhaps lamp-oil has as great a proportion of phlogiston as zinc; yet the flame of zinc, if I am rightly informed, is by much the hottest, and therefore the decomposition in the metal proceeds more rapidly than in the oil.

I USED formerly to think that those bodies would require the greatest heat to begin

* Water.

their combustion, which attracted the phlogiston most strongly; heat weakening the attraction between phlogiston and earth. But that this is not always the case appears from hence, that if this rule held good, those different bodies would suffer equal decompositions in equal times, which does not agree with the last paragraph. In chymistry also, we find that the phlogiston may be transferred from the earth of charcoal, to the calx of zinc; from thence to the phosphoric acid; from the phosphoric acid to the vitriolic; and from thence again to the nitrous acid, or air. Yet charcoal burns with more difficulty than sulphur; and zinc does not begin to flame but with an heat vastly superior to that which kindles phosphorus. Chymistry, however, furnishes us with something analogous to this: thus all the acids, except the phosphoric, attract fixed alkali preferably to calcareous earth; but that acid attracts the earth preferably to the alkali. The vitriolic acid attracts fixed alkali more than the nitrous; but the nitrous acid attracts phlogiston stronger than the vitriolic. But one reason of the greater difficulty in inflaming zinc than
phosphorus

phosphorus * may be, that the former contains extraneous particles which shield and defend the phlogiston from the action of the air: thus water defends phlogiston from the action of the vitriolic acid, and sulphur cannot be formed till by heat, or otherwise, the water is dissipated †. For that the difficulty of the combustion of zinc does not proceed from its fixity, or difficulty of being raised into vapour alone, appears from hence, that the combustion of lead is effected without such vapour, or merely by calcination. But when, by the action of heat, the phlogiston of the zinc is rendered combinable with air, the decomposition proceeds rapidly indeed! by reason of the weak attraction of the phlogiston for the calx, or of the great ease with which the air now attracts the phlogiston from that body.

THE heat attending nitrous combustions may be understood by referring to what was said concerning them in the sixth section. A certain degree of extraneous heat must be ap-

* See also case vii. section VIII.

† Fixable, and phlogisticated air may, for a like reason, be analogous to sulphur, and the volatile vitriolic acid.

plied in order to enable the phlogiston and air of the nitre to combine, and fire is separated from the air by the phlogiston, in the same manner as from the air of the atmosphere in common combustion.

BEFORE oils can be set on fire by the nitrous acid a sufficient degree of heat must be generated by their *chymical mixture** in order to enable the phlogiston and air to unite; and then the combustion and consequent explosion happen as in ordinary nitrous combustion.

THE generation of fixable air is a consequence of combustion; and as this is generated in fermentation, in respiration, and certain other processes †, there must have been a combustion. These combustions take place in the ordinary heat of the air, and are, properly speaking, *spontaneous calcinations*. The heat is less than in inflammation, because of the number of extraneous particles among which it is

* Hence the use of oil of vitriol. The like reasoning may be applied to the combustion of pyrophyrus by common air.

† This is to be understood in cases where fixed air is actually generated, not where it pre-existed and was only expelled as in effervescent mixtures, &c.

shared:

shared : and the light of combustion * is not visible by reason that it is stifled by these particles, and also because it is too rare. Thus fixed air may be produced by heating sulphur in a close vessel ; and by repeating the process, the whole sulphur might, I imagine, in time, be decomposed as effectually as by inflammation, and yet by reason of its rarity, the light shall not be visible. Thus also coal, iron, liver of sulphur, and other substances exposed to air lose their phlogiston in time ; and yet by reason of the rarity of the light, and the slowness with which the decomposition proceeds, neither light nor heat are sensible.

By considering the degree of affinity which phlogiston has for the particles of bodies with which it is combined, the volatility or fixity of these particles, and the nature and quantity of the extraneous particles which enter into the composition of different bodies, we may have the reasons why some bodies inflame with less heat than others ; why they burn more or less rapidly ; why the combustion of some bodies cannot be effected without a continual

* Vide section IX.

application of extraneous heat, and the like. Hence also the difference between inflammation and incineration or calcination. The other phenomena of the second section will easily be understood from hence, and from what has been already said.

WHETHER the hypothesis on which I have proceeded be true or false, experience must determine; those who have leisure and convenience would do well to prosecute the Inquiry. The Reader, on considering the importance of the subject *, will excuse me for detaining him so long with conjectures; and the next section, I hope, will make him some amends. I have only been able to guess at the theory of the heat of combustion; that of the light, at least the observational part of it, I think I can venture to offer as certainty.

* Vide sections X. XI. and XII.

SECTION IX.

Of the Light and Colours which arise on the Ignition, and Combustion of Bodies.

SOME ingenious philosophers have of late found that the calxes of certain metals may be reduced by means of *light*; and that other phlogistic processes may be performed by it: they have therefore imagined, that light is *phlogiston*. The following considerations will perhaps in some measure clear up this matter.

OBSERVATION I. When an incombustible body is heated to a certain degree it emits light; and the light increases as the heat becomes greater.

OBS. II. The light which is first emitted is of a reddish colour, so much so indeed that the body is said to be *red-hot*.

OBS. III. As the heat increafes, the colour verges more towards orange and yellow diluted with white ; and when the heat is very intense, the colour becomes fo white that the body is faid to be *white-hot*.

OBS. IV. But if the heat be ever fo much increased ; yet, if there be no combuftion, the colour is never found to vary from the white towards blue, purple, or violet.

IT is known that light confifts of rays variously refrangible, and that this arifeth from the different fizes of their particeles, thefe rays being moft eafily refracted whole particeles are the fmalleft ; when thefe various particles of light are combined with a body, the leffer ones will be attracted and held moft powerfully, and the larger ones leaft, for the fame reasons that the rays compofed of them are differently refracted by that body. When air is combined with a fubftance, the application of a proper degree of heat will feparate it therefrom, and caufe it to fly off in its elastic ftate. In like manner, when the particles of light are combined with a body, and heat be applied, thefe particeles will

will be dislodged, and expelled from the body by the action of the fire, and as they have a *polar* virtue (as appears by the double refraction of island chrystal) they will take the rectilinear disposition, and constitute *rays of light*. But be this as it may, those particles which are largest, and which therefore are less forcibly retained by the body, will begin to be dislodged with a less degree of heat than the small ones, which the body retains more powerfully; and as these are the particles which constitute the red-making rays, the body must appear red; this red, however, will not be perfect, because some of the other particles also will be expelled, though in less quantity than is sufficient to compose a white: as the heat increases, the orange, yellow, and other particles will be expelled in more equal proportion, and therefore the colour will verge from the red towards white, so that when the heat becomes sufficiently intense, they will all be expelled alike, and the body appear perfectly white. Thus in distillation, when liquids of different volatility are contained in the alembic, if the fire be gentle, the most volatile will come over more pure; and

its purity will be less as the heat is more augmented.

BUT in those bodies which are combustible, we are presented with very different phenomena ; if copper, for example, be heated, it will first shine with a red heat, which will afterwards be whiter according as the heat is increased, as shewn above ; but if it be made to flame, the colour emitted will be green. Zinc may, in like manner, be heated red-hot, and the red colour will become whiter as the heat increases. But if it be made to flame, the colour is intensely white. The flame of sulphur is blue, of tallow, yellow, of lamp-oil, orange ; and there are hardly two bodies whose flames are exactly alike. Incombustible bodies, therefore, when ignited, and also those combustible ones which ignite before they flame, emit light in the following order ; red, orange, yellow, green, blue, indigo, violet. But this is not the case in combustion.

Obs. V. The substance which burns with the least heat of any that we know is phosphorus ;

rus; the colour of its weak flame is a violet-blue, if I am rightly informed. Sulphur burns with an heat less than that of ignition, and its colour (especially when the flame is gentle) is blue; alcohol burns with an heat greater than sulphur, yet below that of ignition, if properly managed, and the colour emitted is blue, though less so than sulphur.

FROM hence it appears, that as in ignition the red-making rays are most copiously emitted (and at first almost entirely); those which are emitted most copiously in combustion are, on the contrary, the violet. To account for this difference, the Reader is requested to attend to the following reasoning.

IT was shewn in the foregoing sections, that heat is necessary to enable the air to attract the phlogiston from bodies; and that the fire separated from the air by the phlogiston, serves afterwards instead of extraneous fire to keep up the heat of the body, and enable the air to continue the decomposition. The fire expelled is so copious as even to heat the body more

than is sufficient to enable the air to attract the phlogiston, as is evident by the flame of a combustible body setting fire to a body which requires a greater heat than it to begin the combustion; the intenseness of the heat, therefore, and the violent attraction of the air, will dislodge the phlogiston from the body faster than the air (especially when it begins to be satiated) can readily combine with it; and those particles which are not immediately combined, attract large atmospheres of æther, which render them incapable of combination with air, and therefore they are driven off in the form of *light*.

Now, as in ignition, bodies retain the blue light most powerfully, and part chiefly with the red, and other particles which compose the less refrangible rays; so in combustion, the air most easily disengages and attracts the larger particles, with which being first nigh saturated, the smaller ones remain behind as the superabundant particles above spoken of; and which, by acquiring ætherial atmospheres, become particles (and are driven off in the form) of light.

The

The flame therefore must appear of a colour on the violet side of white, as we find to be the case *.

BODIES which shine by ignition can, for reasons just given, advance in colour only from red, through a dilute orange and yellow, to white, and can never pass from that white to green, blue, and violet; so neither can the light of combustion pass on to yellow, orange, or red; but yet we find that some flames are tinged with these colours. Thus the flame of a candle is yellow, that of an oil-lamp orange, and of wood red.

BUT it must be observed that the above rule holds good only in those flames whose heat is below ignition. When the heat is intense, the particles which compose the vapour

* The smaller particles will also attract æther faster than the large ones. The reason that light does not thus appear when the vitriolic acid &c. takes phlogiston from bodies seems to be, that by reason that the particles have less atmospheres of fire, they are nearer in contact with the phlogiston, and therefore by attracting it prevents its escape; or the light expelled may be too rare to be visible for the same reason.

are ignited, and the light which proceeds from them is mixed with the light of the combustion; but the light of the ignited vapour is emitted in a contrary order to that of the combustion, the latter beginning at violet, the former at red *; and therefore the mixt colour of the flame will be varied according to the degree of heat, the proportion of the light of the combustion to that of the ignition (which in some cases will depend on the nature and proportion of extraneous particles † in the vapour), and to other circumstances. Thus, the flame of wood seems to be in the first degree of ignition, or *red-hot*, and the proportion of the light, to that of the combustion, is so great

* As a farther proof of the difference observed, those flames which are ignited are opaque; but those that are not ignited are transparent. The flames of oil, and of alcohol, properly managed, will shew this to advantage.

† How these affect the heat of the flame (on which its ignition depends) may be gathered perhaps from the last section; and as the ignition is less, its light is redder, as was shewn above. Different bodies may also, perhaps, have different proportions of the blue light in their composition. Some flames seem likewise not to have any, or but very few, extraneous particles, and therefore are still blue, though their heat be great.

that

that its colour is predominant; the flame of oil, though less red, does not seem to be much hotter than that of wood, but the light of the combustion is in greater proportion, so as to dilute the red to an orange. The same may be observed of the flame of a candle which is of a dilute yellow; and in the like view the colours of flames of other bodies may be considered, some of which I have arranged in the following table.

TABLE

TABLE OF THE COLOURS OF FLAMES.

Bodies arranged according to the degrees of heat necessary to begin their combustion.	Light of the combustion.	Light of the Ignition.	Proportion of the lights of C, and I.	Colour of the flame.
The weak flame of phosphorus	Violet-Blue	None	All C	Violet-Blue
Sulphur	Blue	None	All C	Blue
Alcohol	Greener Blue	None	All C	Dit. a little greener
Small wood	Ditto?	Red-white	I, most	Reddish white
A pitch torch	Ditto?	Ditto?	Ditto	Ditto
Lamp-oil	Ditto?	Ditto?	Ditto, less	Orange-white
Tallow	Ditto	Ditto?	Ditto, still less	Yellow-white
Camphire	Ditto	Ditto?	Ditto, still less	Ditto more white
Nitre and coal	Ditto?	Yellow-white	I, most?	Ditto still more
Copper	Ditto?	Ditto?	C, most?	Green-white
Iron	Ditto?	Whitish	I, most	White
Zinc	Ditto?	Whitish	Ditto, less	Whiter

Others

Others might have been added ; but even those which I have given are very incorrect, being set down only by guess ; and the table is offered merely as a sketch of the subject, to be prosecuted by those who have proper instruments, and other conveniencies.

It is to be noted, however, that different parts of the same flame are unlike in colour : thus, the bottom of the flame of a candle or oil-lamp is blue ; it grows less blue by degrees till it ends in a yellowish white ; but this white, towards the top, verges towards orange, and still further, towards red (especially when the flame is advantageously disposed), till it ends in unignited vapour or smoke,

To understand the reason of this it must be observed, that at the bottom, where the decomposition begins, the light emitted is only that of the *combustion* ; for it takes up some little time to ignite the particles ; and therefore they do not begin to emit the light of ignition till they have ascended some way up in the vapour. But when that takes place, the blue colour of the combustion begins to be changed, till at last
the

the mixed colour in the middle is of a yellowish white. But the light of the combustion being less and less towards the top, till perhaps it quite ceases, and the surrounding air cooling the ignited particles into a less white heat *, the colour of the flame towards that part is more red, till at last the particles lose their shining heat, and pass off in the form of unignited vapour. When the wick is long, and also ignited, the latter phenomena are more conspicuous; for the red light of the wick, and of the particles that escape from it, being mixed with that of the flame, tinctures it, especially at the point, more highly with red. In daylight, or sun-shine, the latter phenomena appear to still greater advantage, the weak light of the combustion being then less capable of interrupting that of the ignition.

THE colour of the light of the combustion of bodies may be known by observing the bottom, where it is as yet unaltered by that of the ignition, for reasons given above. Thus, if you fasten a piece of camphire on a wire, and inflame it, holding it up in the air, you will see

* Hence the Conic form of the flame.

a blue light at the bottom. The light of the combustion in all bodies must be more or less blue, because mixtures of the most refrangible rays produce only various shades of that colour *.

IN the above table only the middle part of the flame is considered, where the light is compound, as in tallow; and only in the weakest state, where it is simple, as in alcohol. But if a quantity of alcohol be burnt, so that the flame rises high, the particles will be ignited. And if we examine the upper part of the flame, and compare the colour with what has been said, we shall find this to be the case. The other phenomena of the lights of ignition, and combustion, either separate, or conjoined, may perhaps be understood by prosecuting the principles above laid down.

COROLLARY I. Bodies retain a considerable

* Perhaps even the strongest ignition that we can cause by our fires does not yield a perfectly white heat. The flame of zinc, however, is intensely white, if I am rightly informed. The light of the ignition is, by that of the combustion, diluted to a perfect white.

quantity,

quantity of particles of light in their pores, or otherwise. These particles are dislodged and expelled from those bodies by a proper degree of heat, and the largest particles most easily, by reason that they are less forcibly retained. Hence the light of ignition.

COR. II. Phlogiston *combined* with bodies cannot be expelled by heat alone, though light can; thus charcoal, heated in a close vessel, though it may be made to emit light, yet is not found to part with its combined phlogiston; yet the light of the combustion is this very phlogiston set at liberty by the combined action of heat, and the attraction of the air. Vide sections VII. and IX.

COR. III. Phlogiston therefore is light in a state of combination with bodies, forming a constituent or essential part of them. Light is phlogiston in an elastic state existing in their pores. As this last is less attracted by bodies, they shine with a less heat: thus electricity and certain phosphori, shine with the usual heat of the atmosphere; and some of the latter, if exposed to any particular sort of the sun's rays,

rays, expel them again in the shade ; that is, the same colour which was forced into the body is afterwards emitted by it, as its attraction for fire, which was diminished by the action of the light, returns.

THAT the light of these phosphori is what I call *the light of ignition* appears by its colour. In some cases of electricity, however, a blue light is observed ; but this does not happen unless a *real combustion* takes place, and that this does sometimes obtain is obvious by an experiment of Dr. Priestley, who made fixable air with this fluid. I had drawn up a theory of electricity, and intended that it should have followed this section ; but found, after I had finished it, that it would swell the volume to a much greater size than was intended. The electric fluid appeared to me to be *phlogiston combined with earth, already more intimately combined with a considerable portion of that principle* ; for air takes it from that earth, as appears by the above experiment ; neither can pure phlogiston combine directly with air, for a reason to be met with in the 7th case of the VIIIth. section. That the earth is of this nature
appears

appears by the sulphureous or phosphoric smell, and by its changing blue infusions red. The quantity of this earth is not sufficient to render the phlogiston coherent; but the difference between pure phlogiston and electricity, seems to be somewhat the same as between pure and fixable air. From case 13th, section VIIth. I had inferred *that as by friction heat is generated, it argues that by friction the attraction between the phlogiston and earth is increased.* Hence when glass is rubbed by the hand, their attractions for phlogiston are both increased, but that of the glass (being the strongest electric) most. The glass therefore will attract it from the hand, and the hand from those conductors which are in contact with it. Yet not the phlogiston, combined in a coherent form in bodies, flows, in this case, to restore the equilibrium or common temperature, but only that which exists in the pores in an elastic state. Pure phlogiston will flow as well as the other; hence the *electrical light of ignition.* By the friction some of this phlogiston will also, perhaps, be converted into electricity, being attracted by the excited effluvia of the hand: and I had gone through all the principal

principal phenomena of electricity. If this sketch of the subject, and the present work be approved, I may hereafter publish the original essay, together with other papers on different subjects.

IN combustion, and some other chymical mixtures, perhaps a small part of the heat may be occasioned by the friction or percussion of some of the particles, though much less than is at present believed : but bodies are probably heated by light *, and by electricity, in great measure by this means ; and entirely by it and the communication of phlogiston, except in cases where actual combustion is caused.

By the second corollary it appears, that phlogiston, when disengaged from bodies with which it was combined in a coherent state, assumes the form of light ; and it was shewn before, that fire when disengaged is the cause of heat. I had formerly run the analogy between these principles farther, and imagined that fire did not only exist in bodies after the manner already described, but that it also

* Hence opaque and dense bodies are most heated by light.

combined with them in a coherent form, was disengaged by phlogiston, &c. and then assumed its elastic state: and also, that it was transferable from one body to another in a fixed state, in the same manner as phlogiston *. But I could not find means to satisfy myself of the truth of these propositions, and the mode of its existence, which I have before supposed seems to agree with the phenomena of heat and cold as exhibited by the sense, and the thermometer; but the truth remains to be cleared up by experiments. In the mean time the hypothesis that “phlogiston weakens the attraction of earth for fire, according to the force of their combination,” and that “the force of their combination may be intended, or remitted,” seem sufficient to account for the phenomena, whatever

* For example: I argued that causticity depended on fixed fire. That a fixed alkali being applied in its mild state to quicklime, the lime combined with the fixed air, and the alkali with the fire. That water expelled fire from the vitriolic acid, quicklime, &c. by means of a superior affinity, as fixed air is expelled from mild alcalis by acids. That spirit of vitriol added to caustic alkali, the acid joined with the alkali, and the fire with the water, the fire being more than sufficient to saturate the water, &c. &c. The like of air. But these things may equally obtain on either supposition, and what respects causticity does not seem to be true.

be the modes in which fire and phlogiston exist in bodies.

To the mode of its existence which I have supposed, it may be objected, that if the particles of bodies have the atmospheres of fire described, the bodies which they compose ought to repel each other like particles of air; for the atmospheres of the particles extending beyond the body, will compose a repelling atmosphere of fire about that body: it may be answered, that these repelling atmospheres are, naturally, balanced by means of electricity. When the latter is removed, the action of the former becomes sensible; for two bodies negatively electrified repel each other; or, if the equilibrium be destroyed in a contrary way, by the attraction in consequence of excitation, or by accumulation of electricity, these repelling atmospheres are equally left at liberty to manifest their action; and the electricity may even conspire with it, if great; hence the mutual repulsion of two bodies electrified positively*.

For

* Electrical repulsion, whether *plus* or *minus*, seems to depend on the atmospheres of fire, as above. But electrical at-

For a reason given above, I cannot now enlarge on this subject, and therefore shall only add, that if the repulsion of the particles of air is not diminished by combustion so much as might be expected, the cause may perhaps partly be discovered from hence; and also by considering that particles of air, &c. may probably be only bodies made up of other particles, these again of others, &c.; and that the atmosphere of a whole particle is made up of these portions of the atmospheres of its elements which extend beyond the whole particle. Hence the more the particle is condensed by cold, or by combination with phlogiston, more fire in proportion will come into the atmosphere from its pores; the fire so expelled will also be more expanded. And contrariwise when the whole particle is expanded by heat, or the loss of phlogif-

traction, on the violence with which bodies attract electricity. Thus, a non-electric being properly presented to a body *minus*, the latter attracts the electricity in the former (and with it the body itself) so violently as to exceed the mutual repulsion by their atmospheres of fire. When the electricity in the two bodies becomes in equilibrio, if the quantities be natural, the attraction and repulsion cease; but if they be still either *plus* or *minus*, the bodies repel each other by means of their atmospheres of fire, as before.

ton,

ton; the repulsion of the whole particle may depend only on the fire in the atmosphere thereof. But if we suppose part of the fire to have been fixed *, the solution is perhaps still more easy.

WHEN air is applied to zinc, and to most other combustible bodies, under proper circumstances, it deprives those bodies of a certain portion of their phlogiston; but another portion remains behind, of which air cannot deprive them. The fixed particles of bodies, therefore, or what the chymists call earth, have stronger attractions for phlogiston than even air, and therefore are *originally* † more pure earth in my sense of the word. The phlogiston which remains, and which cannot perhaps be taken from them by art, is sufficient even to keep them coherent. If they were deprived of that extra phlogiston, they would therefore form a fluid as much more elastic than our air,

* May not the very condensed fire next to the surface of an ultimate particle be said to be *fixed*? Does fire combine with bodies in any other manner than that above described?

† Vide case xvii. sect. VII,

as their attraction for fire would be stronger, if the foregoing reasoning be true.

IN phlogistic atmospheres, the smallest particles, being most attracted, may immediately surround the particle; those which are larger next above, and so on to the largest, which may compose the external part of the atmosphere. Hence may be another reason why air in combustion first, and most easily attracts the larger particles, as observed before. Hence also perhaps the reason that the spheres of cohesion are so limited. The like atmospheres may be supposed of the elements of a proximate particle, of the elements again of these, &c.; the latter will retain their phlogiston more powerfully than the former, as being less; and their cohesion will, for the same reason, be stronger. This, were it true, might enable us to account for what was observed in the last paragraph; also for the heat generated by oil of vitriol and water, &c. and (together with what was observed of the manner in which fire is combined) gives us some idea of the internal structure of bodies.

ASTRONOMERS freeze at the thought of the planet Saturn, and entertain a contrary sentiment with respect to Mercury ; but if the proportions of fire, &c. in the different planets be properly adjusted, as their densities seem to shew, it may not be so hot in Mercury, nor so cold in Saturn as is at present believed. Whether the like reasoning may be applied to the sun and fixed stars ? (See what was said above concerning phosphori.)

SECTION X.

Of Respiration, and Animal Heat.

I FIND by some extracts from Dr. Priestley's publications, that that great philosopher has demonstrated *that the use of respiration is to carry off the phlogiston which the blood acquires during its circulation in the body.*

IT seems to appear that there is a very close analogy between respiration and combustion; and this has been an ancient observation. Dr. Willis of the last century, treating on the heat of the blood, has this passage. "Though it seems an hard saying that *the blood is accended*, yet seeing we can attribute its incalcescence to no other cause, why should we not impute it to this? especially seeing *the proper passions of fire and flame* agree to the *life of the blood.*

"FOR the chief and most essential requisites to continue a flame are these three: 1st. That
a free

a free and continual access of air be granted to it as soon as it is kindled. 2dly, That it enjoy a constant sulphureous *pabulum* or fuel. And 3dly, That as well its fuliginous as thicker recrements be continually amended from it: and seeing these agree to the *vital flame*, as well as to an elementary, it seems very rational to affirm that *life itself is a kind of flame.*"

THIS learned and very ingenious physician saw plainly that there was an analogy between combustion and respiration; and between the heat of flame, and that of the blood. But for want of proper discoveries concerning the nature of combustion, &c. his ideas were more confused and obscure. A little more light may perhaps be thrown on the subject in the course of this section, and yet succeeding authors, who push their inquiries farther, will make a similar observation on what I have done.

ACCORDING to the discovery of the great philosopher above mentioned, the blood which is brought to the lungs from the body, contains a greater quantity of phlogiston than that which goes from the lungs into the body. And the

the air takes this phlogiston from it in its passage through the lungs.

It appears, by what was said in the Vth. section, that when phlogiston and air combine, heat is generated. And if the conjectures in section VII. can be depended on, the heat arose from hence, that the phlogiston by combining with the air weakened its attraction for fire, which therefore gravitated towards, or was attracted by, the bodies around, till an equilibrium, with regard to the attracting powers of the respective bodies, again took place. When therefore the particles of air combine with the phlogiston which they attract from the blood in the lungs, heat will in like manner be generated : that is, the attraction of the particles of air for fire will be diminished as in combustion. The blood which is carried to the heart therefore will be hotter than that which is brought to the lungs ; and hence one cause of the heat of the blood.

BUT when the particles of blood lost their phlogiston to the air, their attraction for fire, which by case 7th. section VIIth. was weakened
by

by the phlogiston, will, now they are deprived of that principle, be again increased. A part of the superabundant fire, therefore, will be attracted by those particles of blood; and they will be carried in this state into the body.

It is known to philologists, that if the nerves which serve any particular part be destroyed, that part will be colder than before, notwithstanding that the blood circulates through it as usual. Now as the blood, when it entered that part was already hot, and as, before the nerves were destroyed, the heat of the blood was supported in its passage through that part, and on the contrary, when the nerves were destroyed, the blood was cooled in its passage through it; it follows *that the heat of the blood is supported in its passage through a part, by means of the nerves by which it is served.*

THE heat in combustion seemed to arise from hence; that when the particles of air combined with phlogiston, their attractions for fire were diminished: may not the same reasoning be applied to the blood? as the blood in the veins of the body is found to contain
more

more phlogiston than that in the arteries; and as the heat of the blood in the body appears to depend on the nerves, may we not argue in the manner following?

THE blood in its passage from the arteries to the veins has phlogiston imparted to it either immediately or mediately, by the nerves. But each particle of blood thus combined with phlogiston will have its attraction for fire lessened, analogous to what happens to a particle of air in combustion. Heat therefore will follow for the same reason that it follows on the combination of phlogiston with air in combustion, only in a less degree. The particles of blood thus phlogisticated, and rendered unfit for the further purpose of causing heat, pass on with the circulation, and fresh ones succeed. When the phlogisticated particles arrive at the lungs, they are decomposed by the air which attracts their phlogiston, and from which the particles of blood, in return, take a quantity of fire, so that they are again rendered fit for the purpose which has been described. And this seems to me to be the manner in which the blood becomes hot.

SECTION XI.

Of the vital and other motions of the Body.

IF some purpose of the last importance to the animal had not been designed by *respiration*, the all-wise Author of nature would, certainly, not have rendered life so dependant on that process as to be incapable of existing, even a few minutes, without it.

By the last section it appeared probable, that the heat of the blood depends on the *nerves*; or that the phlogiston which the arterial blood acquires in its passage to the veins, is communicated to it by those organs.

ALL the vital motion or functions of an animal body are performed by means of the nerves; and all those functions may be reduced
to

to the *contraction of the moving fibres* *. I would say, therefore, that *for a nerve to cause the action of a fibre, it is necessary that the nerve should impart phlogiston, either immediately or mediately, to the blood flowing through, or by that fibre.*

THAT there is a connection between the action of the fibres, and the phlogistication of the blood, appears, I think, by the following considerations. I. The heat of the blood depends on the nerves, as appears by the last section. II. According as more of the voluntary muscles act, or as their action is stronger, more blood is phlogisticated in a given time; for the heat generated is greater, and the respirations are quicker: and III. The motion of the blood through a muscle is known to be as necessary to its action as the nerve; for if the artery be tied, the muscle becomes paralytic as effectually as if the ligature had been made on the nerve.

THE action of the moving fibres may be divided into voluntary, and involuntary: some

* Sensation is not here considered.

fibres serve for involuntary action alone; others for voluntary; but those muscles which are for the voluntary motions of the body, are continually exerting involuntary action. The contractions of the arteries, the veins, and other vessels of the body for the purpose of circulating the fluids, &c. are performed by means of moving fibres. The muscles, membranes, coats of vessels, &c. are made up of such fibres; there is no sensible part of the body but what abounds with them; all these are continually exerting involuntary, and most of them in walking, voluntary actions, necessary to the life and well-being of the animal. Now, as there seems to be a mutual dependence between these actions, and the phlogistication of the blood, as the number of particles of blood is not infinite, but on the contrary, only such a quantity can be admitted into the structure of the animal fabric as is sufficient to balance the action of the solids, if there was no contrivance for dephlogisticating the blood, the whole mass would soon be rendered unfit for the purpose just described, as well as of communicating heat, and death would presently ensue. Nature has therefore

fore provided the animal with lungs ; the blood, phlogistified as already related, is conveyed to that organ ; the air in inspiration restores it to its original purity by taking from it its phlogiston, and furnishing it in return with fire, and thus renders it again fit for the purposes of animal motion and heat. In proportion therefore as the sum of the whole action of the fibres of an animal is greater, that is, in proportion as a greater quantity of blood is phlogistified in a given time, the motion of the blood ought to be increased, and the inspirations of air more frequent, in order that the restoration of the blood to its former purity, may keep pace with its phlogistification in the body.

Now, as life depends on the action of the fibres, as above, as there is a necessary connection or dependence between the action of these fibres and the phlogistification of the blood ; and as from the great number of moving fibres in the body in continual action, and the small quantity of blood, the latter will be presently phlogistified, we have an idea of the very great importance of respiration, and the absolute

lute necessity of it to the continuance of life, as we find by experience to be the case; neither the *heat of the blood*, nor even the *vital motions of the system* being capable of existing long without it.

SECTION XII.

Of the Action of the Fibres, or muscular Motion.

WHAT has been said in the two last sections may be allowed, perhaps, to be in some degree probable. I shall give no opinion with regard to what follows.

THE idea of muscular motion, which I had formed to myself many years ago, was, that *by the influence of the nerve, the particles which compose a muscular fibre had their attractive forces increased, so that they were drawn nearer together, but that as soon as that influence ceased, the increase of attraction vanished, and the particles receded to their previous distance from each other.* I had contented myself with a theory in the abstract; but Dr. Priestley's admirable discovery will, perhaps, enable me to assign the physical cause of this contraction.

IT

It appears probable to me, after an attentive consideration of the subject, that the matter or fluid contained in the nerves which serve for motion, is *the phlogiston, combined in a coherent form with an earth already more intimately blended with a considerable quantity of that principle; so that their combination is but weak* *. Those who have read the seventh section carefully will comprehend my meaning by this definition, and therefore I need not comment upon it. This matter does not seem to be derived from the nerve into the fibre of itself, or by propulsion, like the blood, for if the nerve be tied, it does not swell between the ligature and the brain. The matter of the voluntary nerves is, I think, only driven down by the will †. That of the involuntary ones is obtained by means of the pulse of the arterial blood, and other stimuli in the body, by *irrita-*

* Some phenomena seem to shew that the latter ingredient only is secreted by the brain, and that the former is imbibed from the stomach, &c. That the nervous fluid is not the electrical matter, as some have supposed, is plain from its not combining with the blood in the manner the phlogiston in question is found to do.

† Pain is a stimulus to these nerves; but then it is by its action on the sensory, &c.

tion, or reflux. Hence perhaps one reason * why the blood does not move in a smooth uninterrupted course in the arteries, but by pulses; and hence also the reason that it does not flow by pulses in the veins, the fibrous mechanism terminating where the veins commence, so that there is no further occasion for it. The reason of all this seems to be, *that such a quantity of matter only may be occasionally derived from the nerves, as may be necessary for the purposes of the animal economy, &c.* which, therefore, is left to be regulated by the will, by the pulse of the blood (the force of which depends on muscular action), by heat, and other stimuli.

WHEN by the pulse of the blood, the influence of the will, &c. a portion of this matter is derived from a nerve into a fibre, it seems to me that the particles of which the fibre is composed, having a greater attraction for the phlogiston, than the earth has with which it is already combined, take the phlogiston from that matter, and thereby have their force of cohe-

* The other reason seems to be that the fibres may be put into vibrations, by means of which the effect mentioned is also probably produced. Of these vibrations I may speak more at large in future.

sion

sion increased; the fibre, therefore, will contract: but the particles of blood flowing by the fibre, and having a still greater attraction for phlogiston, takes it immediately from the fibre, which therefore is again relaxed. Hence, as the contraction of the fibre is but momentary, if its contraction be required to be continued for a given time, there must be a continual derivation of matter into it from the nerve during that time *.

It may be asked, that if muscular motion be performed by means of phlogiston causing a temporary increase of attraction or cohesion in the particles of the fibres †, why this indirect method

* I endeavoured to account for muscular motion by the phlogistication and consequent contraction of the blood only, and also by the æther disengaged from the phlogiston by the stronger combination. (Vide section VII.) But neither of these by any means answer to the phenomena. It may be observed that, probably, only the crassamentum, or its coagulable lymph, attracts the phlogiston from the fibres. That phlogiston when combined brings particles nearer to each other is plain by its effect on air, metallic calxes, &c,

† If an artery be compressed, a sensation of warmth is perceived in the part which it serves; but as the blood returns, cold is felt. The warmth arose perhaps from the fibres being

method was adopted, and why the nerves were not furnished with it as a fluid, so that it might have been derived from them immediately to the fibres? It may be answered, that probably phlogiston cannot be managed thus *per se*; and if it could, yet the quantity which a nerve would contain would not perhaps be sufficient for a single contraction of a muscle: whereas, by this contrivance, a nerve can contain a sufficient quantity to last a long time. But there is, probably, still another reason; it has been an opinion of long standing that the parts of the body are nourished either wholly, or chiefly, by the nerves; for a part rendered paralytic by dividing a nerve wastes, notwithstanding

phlogisticated, and the blood not being able to take the phlogiston from them, by which their attractions for fire continued diminished. But when the blood flowed again, and attracted the phlogiston from the fibres, cold must have been the consequence, by the theory of combustion before explained. Also, when the artery only is compressed, the fibres seem to be more rigid or contracted than naturally. But when only the nerve is compressed, the fibres seem, on the contrary, to be more relaxed. If the experiments which I have made on myself (of which these conclusions are the result) can be depended on, they furnish a kind of proof of the theory of muscular motion above laid down. It is also known that a muscle does not swell when it contracts.

that

that the blood flows through it as usual. If the foregoing conjecture be true, the nervous matter is a compound of phlogiston, and an highly phlogisticated earth ; and each of these ingredients may have their respective uses. The use of the phlogiston may be to cause the contraction of the fibres, and the heat of the blood. that of the other ingredient (the phlogisticated earth) to nourish the fibres, &c. not perhaps alone, but conjointly with the blood ; and hence the attraction of the fibres for phlogiston is between that of the nervous earth, and the blood. Hence also the gelatinous nature of the fibres. Hence people who use no exercise have their flesh more delicate and fat than those who labour hard, the nervous matter of the former not being so liable to be carried off, but enters more into the composition of the fibres *, and some of it, perhaps, even in an undecomposed state. In the hands of a Pringle, or a Fothergill, these observations, and others which have been given, might, perhaps, be rendered of use in the practice of physic. It may be added, that as oil of vitriol cannot decompose char-

* Hence the necessity of rest, or sleep appears,

coal, so blood may not be able to decompose the nervous matter itself, though it so readily takes its phlogiston from the fibres. The nervous medulla is not easily combustible by *air*, if I remember right, notwithstanding that it is so readily decomposed by the moving fibres, if the foregoing conjectures be true.

It may also be objected, that a longer time seems necessary for this process than appears to be consistent with the instant contraction of a muscle from the influence of the will. But not to mention how quick the transition of so subtle a principle as phlogiston may be effected, I could, I think, clear up this difficulty by quotations from papers on the subject; but as that would lead me too far out of my way, I shall only observe, that the perception which we call *willing*, and which we usually consider as the cause of the action of a voluntary muscle, is only an effect of the same cause in the sensory, by which the contraction of the muscle is brought about. And, to illustrate the result by a simile, as when a man is shooting at a mark, and we stand near that mark at a distance from the man, the shots are heard to strike against the pa-
per

per as soon as the report of the gun ; so, for a reason somewhat similar, the volition and the action, seem to us to be in a manner cotemporary. It may likewise be remarked that our perceptions and actions are exceedingly slow, when compared with the action of the more subtile principles of nature, as I may hereafter explain, if the present work be approved.

A CERTAIN degree of heat, though necessary to a particular species of animals, is by no means so to the animal functions, or to animal life in general. Thus fishes are as perfect in these respects as quadrupeds, though their heat be much less. The heat is necessary to the liquifaction of the blood, and, perhaps, of the nervous compound ; and also, to enable the fibres the better to decompose that compound, and the blood again the fibres. It may hereafter be shewn that it also probably assists in the vibrations of those fibres. The blood of fishes is fluid with a degree of heat in which that of quadrupeds would be congealed. Animals which require much heat to keep their blood, &c. sufficiently fluid, decompose a proportionally larger quantity of air, so that their blood
may

may be more heated in the lungs, and also that its particles may carry a greater quantity of fire into the body to be extricated by the phlogiston from the nerves. Fishes, whose heat is required to be but little, decompose a smaller quantity of air, in an equal time than quadrupeds; and the air which is separated from the water by their gills, and again purified by water, may be sufficient for that purpose. Now as less fire is separated from the blood of fishes than from that of quadrupeds, it argues that less phlogiston is also imparted to the blood by their nerves: and this agrees with an observation of physiologists, that *the fibres of cold animals are more irritable than those of hot ones*. The balance therefore is preserved; for as less heat is required to liquify their blood, &c. than in land animals, so less *phlogiston* is necessary to the contraction of their fibres.

IT would be easy to enlarge on such a subject as this; but as I only offer what has been said as speculation, and by way of hint to be prosecuted by others, I shall not *here* pursue the idea any farther. I will only add a wish that what I have offered, may not give occasion

sion to the barbarity of making experiments on living animals. Were I even certain that this theory could be proved by making such experiments, I would not attempt them, as I do not think we are by any means warranted in putting animals to torture to gratify philosophical curiosity: I would not be understood as speaking this from a principle of superstition; it is dictated by my own feelings: that man who has experienced in himself the extremity of *pain*, must be something worse than I can imagine to inflict it on animals, who are incapable by their natures of giving him cause.

A P P E N D I X.

SINCE the foregoing work was finished, a book has been published on the subjects of animal heat, &c. by Dr. Leslie. The very favourable account which the Reviews gave of that work, and the deference which I paid to the judgment of the authors of them, made me at first doubt whether I had not proceeded entirely on wrong principles in my inquiries on the same subjects, and had resolved to withhold either the whole Essay on Combustion, or at least that part of it which treats of respiration, &c. from public view; but after considering the matter more attentively, and comparing the different performances, I thought I discovered reasons for imagining that my arguments were not so fallacious as I had at first concluded. I

was

was at length so far satisfied of this, that I drew up a paper by way of refutation of Dr. Leslie's theory, and intended to have subjoined it to my Essay ; but after the work was in the press, another performance on the same subject was put into my hands, written by Mr. Crawford *, which contained experiments totally subversive of Dr. Leslie's principles, and therefore rendered my refutation, which was chiefly speculative, needless.

WITH respect to the latter performance, I do not hesitate to pronounce it one of the best philosophical pieces that the present age has produced. I have no knowledge whatever of the Author, any more than I have of Dr. Leslie, and therefore can have no other motive for praising his work than a sense of its merit : I read it with pleasure, not only on account of the new and important points of philosophy which it unfolds, but of the truly ingenious and philosophical manner in which he has treated his subjects. We are here presented with a specimen of the true method of investigation in philosophical

* I had not the pleasure of seeing this excellent work till some time after it was published.

matters, and with an example of that becoming modesty which always accompanies real genius.

THIS justice which I do to Mr. Crawford's performance may be considered as the more sincere as I frankly acknowledge that the pleasure with which I perused it was not unaccompanied with pain. I had been writing on the same subject; had advanced a step farther than others had done, and was about to make my discovery public. Had I been fortunate enough to have gone to the press before the ingenious Author I am speaking of, a small degree of fame would probably have been acquired by the publication: but this Gentleman has outsoared me; he has completed the subject which I had only begun, and proved by facts, propositions which I had only offered as conjectures. Unhappily I have been so circumstanced that it has not been convenient for me to engage in a course of experiments requisite to the complete investigation of a philosophical truth. I could only argue from those facts which had been published by others, and from analogy; yet by the help of these I was enabled to discover that combustion is a truly chymical process,

cess, and that it depends on the superior affinity or attraction between phlogiston and air : that by the combination of these, a degree of heat is generated sufficient to account for the heat and light of flame, and for the continuance of the combustion after once begun. But the origin of that heat I had only dreamt of ; and even my dream I find did not wholly correspond with the truth : yet had I published first, I should perhaps have deprived our illustrious Author of some part of the glory which he has gained by his excellent performance : I should at least have had the credit of discovering the propositions established in the third and three subsequent sections of the Essay, and perhaps of furnishing the *hint* of the remainder of the subject. but Mr. Crawford has fairly got the start of me.

THIS, however, is not the only instance of two persons unknown to, and ignorant of each others pursuits happening to hit upon the same discovery. That mine were made independant of that Gentleman's, appears by a view of the two performances ; for it will be seen that we proceeded on a very different rationale, and that

that we arrived at the same conclusion by direct opposite roads. My learned and worthy patron, whom I have also the honour to call my friend, will do me the justice to acknowledge that my Essay was in his hands, and that I had agreed with the bookseller for the publishing of it before Mr. Crawford's, or even Dr. Leslie's performance appeared.

I AM weak enough to confess that I should like to have had a share in the honour of this discovery, and perhaps the candid in the learned world will not refuse me some credit on that head, on a review of the evidence before them. Yet I should not have published this Essay after having read Mr. Crawford's treatise, but ~~that~~ it was already in the press, and that there are a few points in which I had gone perhaps farther than that gentleman, or in which he does not seem to be sufficiently clear.

THE general cause of combustion *, for ex-

* It is worth while to observe that extraneous fire may be said to heat bodies *positively*; phlogiston, friction, &c. *negatively*.

ample, as far as relates to heat, is elegantly assigned by the learned author ; and the different proportions of fire which he has shewn to be contained in fixed and atmospherical air, are far greater than I imagined *a priori* *. The light, the different colours of the flames, and some other particulars which he has not attended to, may perhaps be understood, in some measure, from my Essay.

“ IT is probable,” says the ingenious author, “ that the vapour of pure nitrous acid contains as much absolute heat as atmospherical air ; for the power of the former in maintaining flame is nearly as great as that of the latter. In the deflagration of nitre, the acid is converted into vapour, which being the same moment combined with the phlogiston of the coal, the fire is

* Had I measured the degrees of heat before, and during the combustion, and noted the quantity of air consumed, this difference might in some measure, have been discovered ? but through want of conveniencies, I was obliged to content myself with a solution in the *gross*. The difference of elasticity, &c. in fixed and atmospherical air, I confess, does not seem to answer to their quantities of fire, as discovered by Mr. Crawford ; perhaps some of the suggestions at the end of the VIIth. and IXth. sections will better account for it.

instantly

instantly disengaged, an elastic fluid is generated, and a loud explosion produced." That *air* was really contained in the nitrous acid previous to the combustion *, is evident from the fixable air which is generated. For if the vapour had been merely of an aqueous nature, though it might have been expanded by the heat, it would have been condensed into an aqueous liquid, and not fixable air, when cold †: and this being admitted, the Author's supposition, that "the vapour of pure nitrous acid contains as much absolute heat as atmospherical air," will appear to be true, and is a farther confirmation of his excellent theory.

Mr. CRAWFORD has proved that phlogiston and fire are different fluids, contrary to what has hitherto been imagined. I had attempted to shew that *æther* was a third fluid, different from both these.

Our Author imagines the attractions of bodies for phlogiston to be proportional to the degrees of heat necessary to begin their com-

* Vide section VI.

† Vide section VII.

bustion, and I once fell into the same error ; but that this is not always the case will, I think, appear by what is said on that subject in the VIIIth. section of my Essay. May not the differences there observed be partly owing to the different states of the double affinity ?

I HAD endeavoured to shew that phlogiston diminishes the attraction of bodies for fire, in proportion to the force of its combination ; and that this force of combination is capable of being intended or remitted even in the same body. But of the latter of these propositions, Mr. Crawford does not seem to have been apprized.

THAT Gentleman, in one part of his admired performance, has run a comparison between fire and fixed air ; but fixed air is not regulated by an equilibrium, or common temperature, like fire *, neither does it appear that fire exists in
a loose

* Fire does not seem to cohere with, and form an essential part of bodies like phlogiston, and fixed air. If the phlogiston of a metal, or the fixed air of marble be taken from them, the nature and constitution of those bodies are quite altered, or they are decomposed ; but this does not happen with regard

a loose or separate state like that fluid, but is retained by the particles of bodies according to a certain law, if my reasoning be just. The affections of the sense, and of the thermometer by heat, are not, I think, conceivable but by admitting that law. (Vide case xi. sect. VIII.) When phlogiston is added to air, the fire, according to my idea, is not *expelled* *, or does not *fly off* in the manner of fixed air from bodies; it would still be retained by the air, though in a less forcible manner, if there were no other bodies near, or bodies by which it was not more powerfully attracted. If the bodies around were hotter, it would even attract fire from them; but this is spoken with submission to better judges.

IN regard to animal heat, I find by our excellent Author's experiments, that I had fallen

to their fire, at least there does not seem to be the same kind of analogy as between phlogiston and fixed air. There seems to be a greater analogy between electricity and fire in this respect.

* I have used this word in many places, but in such a manner as to carry with it the above meaning.

into an error in imagining that the blood is partly heated in the lungs *. *That heat however is generated by the decomposition of air in that organ*, appears from hence, that the air which is expired is hotter than that which is inspired; and also by the following quotation from our Author's work. "By the heat of the surrounding medium, the evaporation from the lungs is increased. Now it may be shewn, that if the evaporation from the lungs be increased to a certain degree, the whole heat which is separated from the air will be absorbed by the aqueous vapour." And by the converse, if the evaporation from the lungs be diminished, the

* Mr. Crawford in his first proposition affirms, that air is fitter for respiration in proportion to the absolute heat which it contains. But it ought to be observed that air is rendered unfit for respiration by other means besides phlogistication; as by particles floating in it which irritate the lungs, and the like: thus, in combustion of sulphur, the acid is as prejudicial in this respect as the phlogiston. This consideration does not seem to have been sufficiently attended to of late; but asthmatic people daily experience the justness of the remark. Pure fixable air does not kill by irritating the lungs, but by not carrying off the phlogiston of the blood. The salubrity of air therefore cannot be determined by the eudiometer alone with sufficient accuracy.

blood

blood will be heated. But in general the evaporation from the lungs is so proportioned, that the heat of the blood is not increased in passing through that organ : my error arose from not attending to this circumstance, which indeed could not have been known but by experiment.

THE only instance, of moment, of our admirable philosopher giving into hypothesis is with respect to the origin of the phlogiston imbibed by the blood ; he supposes that it is taken “ from the putrescent parts of the system.” But I see no reason why the phlogiston from these parts may not be discharged either wholly, or chiefly, by perspiration, and by urine ; and that it is so, seems apparent by the very great quantities which these excrements, especially the former, contain. Neither does the necessity of the elaborate, and (to all appearance) very important process * which he had been describing†, on this supposition, ap-

* Respiration, &c.

† Hence Mr. Crawford is at a loss when he comes to apply his principles to cold animals.

pear. I may have been prepossessed in favour of my own hypothesis, and therefore may not be a proper judge of this matter: as a proof of it, I shall not scruple to confess that I felt myself pleased on finding that Mr. Crawford, in this part of his work, deviated from my track; I shall therefore leave the merits of the two hypotheses to be determined by the more impartial Reader, or by Mr. Crawford himself; for I have conceived so good an opinion both of his judgment and candour, from his admirable performance, that I would cheerfully acquiesce in his determination.

HAVING taken the freedom to point out another's errors, I should next proceed to enumerate my own. The task, however, would now not only be laborious, but useless, as my theory was only given by way of conjecture; though, for the contrary reason, it was proper to notice any error of Mr. Crawford. Some of my mistakes I have already mentioned; the following, though not shewn to be such by Mr. Crawford's experiments, were yet discovered by a more strict review of the foregoing Essay in
consequence

consequence of that Gentleman's very ingenious publication.

IN the VIIth. section I adopted Dr. Black's supposition, that *phlogiston has a centrifugal tendency*, and thought I had accounted for it by imagining that its particles attracted æther. But though this should be allowed, yet unless the globe of the earth did also, there would be no repulsion between them. Particles of earth gravitate because æther is mutually repelled by them, and by the terrestrial globe; and, therefore, if phlogiston attracts æther, there can, at most, only an indifference be produced in it with respect to gravity or levity, the globe of the earth, and the particles of phlogiston mutually destroying each others effects. The rays of light, setting aside their *inflection*, which may be otherwise accounted for, do not seem to have either centripetal or centrifugal tendency; or if they have either it does not appear to be in any considerable degree: the like may be observed of electricity, which I take to be phlogiston in the next degree of purity to light. If fire attracts æther, fire also must be alike indifferent

ferent with regard to gravity or levity, and this seems, by experience, to be the case. There are however, methods of conceiving how phlogiston may diminish the gravity of bodies; or do metals, &c. attract air, when calcined, in lieu of their phlogiston, and thereby have their weight increased? for an effervescence attends the reduction of those calxes *, as I find by the Chymical Dictionary: should this latter be the case, it would appear (and it would be a little extraordinary) that *error* had led to me *truth*.

THE above, as it has accidentally been the source of right, so it has also of wrong reasoning, as may be seen in the course of the VIIth. section. I do not however find reason to reject the general system suggested in that section; and am still inclined to think that *Æther*, *Fire*, *Phlogiston*, and *Earth* are the four principles of

* Dr. Priestley shews that dephlogisticated air may be expelled from the calx of lead. The dephlogisticated air may have been formed of fixed air which the calx had attracted, and afterwards decomposed. (Vide § VI.)

which

which the world is composed, (taking also into consideration what was said of their properties in page 127): but the following seems to be a more proper arrangement of them, and of their uses. *Æther*, and earth, are mutually repulsive; hence the gravitation of the particles of the latter: fire and phlogiston are principles intermediate to those; the latter seems to be the principle of cohesion among the particles of earth; the former of their separation. Phlogiston is also the cause of light, fire of heat, and on the various compositions or associations of the above principles, the sensible qualities of bodies, and the phænomena of nature in general, seem to depend.

As I formally renounce the false reasonings which may be met with in the VIIth. section, and some other parts of this Essay, candour, I presume, will prevent their being brought in judgment against me.

If any considerable part of the foregoing work should have the good fortune to be approved, the errors which I have discovered, and
any

any others which may in the mean time appear, would be omitted in a future edition. They had certainly been so in this, as well on my own account as the Reader's, had I seen Mr. Crawford's performance in time. The greatest philosophers that ever lived have fallen into errors *, especially where experiments where wanting to ascertain the truth; it is no disgrace to err in such good company, and as I make the critic my *priest* by confessing to him my *faults*, I have a firm *faith* in his supporting the christianity of the character by granting me *absolution*.

* The recent instance of Dr. Leslie on the same subject, a gentleman who had every advantage over me in point of information, might be urged; and also the opinion of the gentlemen concerned in the Reviews on his performance.

P. S. It

P. S. Does not Dr. Priestley's discovery of "light decomposing fixed air in water." depend on the principle laid down in the XVth. case, and applied to friction, percussion, &c.* at the end of the IXth. section? are there not in the water particles, either of the water itself, or of more fixed substances, which have originally stronger attractions for phlogiston, than the particles of air? do not the rays of light, by their action on those particles, increase their attractions for phlogiston, and thereby enable them to take it from the particles of air contiguous or perhaps in combination with them, agreeable to the principles above alluded to? hence heat has not this effect. It seems therefore to be analogous, in principle, to the decomposition of fixed air by agitation with water, a former discovery of that excellent philosopher.

I HAVE supposed in page 195, that the phlogiston is derived to the nerves, not from the

* That is, to the putting the particles of bodies into vibrations; though the heat is not the immediate effect of these vibrations, as has been imagined, but as explained in section IX.

brain, but from the chyle in the various parts of the body ; as the chyle is agitated in and against the vessels, may not this be brought about on the same principle? Dr. Leslie advanced a proposition, that “ by the action of the vessels, the phlogiston of the chyle is gradually evolved throughout the body.” There seems to be some truth in the opinion, though that Gentleman, imagining phlogiston and fire to be the same, erred in the consequences which he drew from it. If the above be the true state of the case, and if what is advanced in the three last sections of my Essay be just, the phlogiston is transferred from the chyle to the nerves, from the nerves to the fibres, from the fibres to the blood, and from the blood to the air. Some other operations of nature may probably depend on the same principle.

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